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RELIQUIAE PHILOLOGICAE:

OR

ESSAYS IN COMPARATIVE PHILOLOGY

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RELIQUIAE PHILOLOGICAE:

OR

ESSAYS IN COMPARATIVE PHILOLOGY

BY THE LATE

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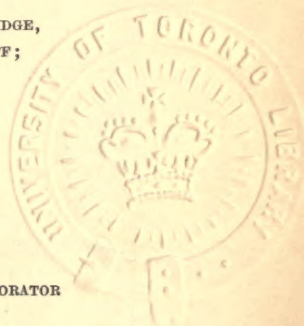
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IN MEMORIAM.

HERBERT DUKINFIELD DARBISHIRE, the son of Mr Herbert Darbshire, was born at Belfast on the 13th of May, 1863. His mother's maiden name was Bruce, and he came of a good stock, both morally and intellectually. He received his early education, for nearly seven years, at the Royal Academical Institution in his native town. He afterwards entered the Queen's College, Belfast, where his career began in 1880 by his winning the Sullivan Scholarship, and ended in 1883 with his attaining a Senior Scholarship in Greek, Latin, and Ancient History. In the same year he obtained a first class with honours in Classics in the examination for the degree of B.A. in the Royal University of Ireland. In October, 1884, he came into residence at St John's College, Cambridge. He had already given good proof of his proficiency in Classics at the examination for Entrance Scholarships, but want of practice in Verse Composition prevented his attaining the place to which his general merits might well have entitled him. To the same cause it was due that, when he presented himself for the first part of the Classical Tripos at the end of the second year, he was placed in the second class, though in the first division of that class. Two years afterwards, in 1888, he was in the first class of the second part of the Classical Tripos, the subjects for which he obtained that position being classical scholarship and comparative philology. Meanwhile he had been elected to a foundation scholarship. In January, 1889, he was promoted to a McMahon law studentship, which he held for the full term of four

years. He read for the Bar in the chambers of Mr J. G. Butcher, late Fellow of Trinity and afterwards M.P. for York. In November, 1892, he was elected to a fellowship at St John's, and was called to the Bar shortly after.

During his University course he had devoted much of his time to the study of Greek philosophy, but it was as a comparative philologist that he showed the highest promise. Several of his papers were published in the Transactions of the Cambridge Philological Society. His "Notes on the *Spiritus Asper* in Greek," together with some contributions to Greek lexicography (*ἐπιδέξιος*, *ἐνδέξιος*, &c.), appeared in 1890; and his paper on the Indo-European names for Fox and Wolf, in 1892. To the *Journal of Philology* for 1888 he contributed an article on the "Numasios Inscription," and to the *Classical Review* (1892) a paper on "Abnormal Derivations," besides several important reviews. He was also a frequent contributor to the *Athenæum*.

In 1891, when the Readership of Comparative Philology at Cambridge was vacated by the resignation of Dr Peile, Mr Darbishire was urged to be a candidate for the office; of all the candidates, he was the youngest, but he was acknowledged by competent authorities to be also one of the ablest. He had already begun to make his mark as a philological investigator and as a teacher. As a private tutor, during several Long Vacations, he gave courses of lectures on the Elements of Comparative Philology, which were highly valued by those who had the privilege of attending them. In 1893 he arranged for coming into residence in the Long Vacation with a view to giving another course of lectures on the same subject. He had recently gone to Hunstanton for a change of air, and during his absence had caught a chill which was followed by an attack of pleurisy. He was, however, recovering from this, when a sudden and unexpected hæmorrhage from the lungs took place, and he died in a few minutes. Dr Donald MacAlister, Fellow of St John's College, who had attended him in his illness, was alone with him at the time of his decease. The date of his death was Tuesday, July 18, 1893.

The first of many touching tributes to his memory came

from the Principal of Girton College, who wrote as follows on hearing the announcement of his death :—

We have seldom had a lecturer who has inspired his pupils with greater admiration for his methods and greater confidence in his knowledge ; and even those who have known him for a short time only, feel that they have sustained a great loss in his death.

I quote the following from the *Athenæum* for July 29 :—

He was one of the most promising, if not the most promising, of British comparative philologists, and might have been expected to found a new school. His papers published in the *Transactions* of the Cambridge Philological Society and in the *Classical Review* display singular acumen and originality, together with a thorough grasp of sound scientific method ; his separately published ‘Notes on the *Spiritus Asper* in Greek’ is quite a model. Mr Darbishire was also an excellent classical scholar and critic. His very attractive character was ennobled by the modest dignity and cheerful courage with which he bore serious physical disadvantages entailed by an accident during infancy. His intellectual power and brightness, his rare charm of manner, his wit, and his genial mood, made him a delightful companion and he was a prime favourite with children.

I append an extract from Dr Postgate’s notice in the *Academy* of the same date :—

(His dissertation entitled ‘Notes on the *Spiritus Asper*’) “was a very remarkable performance ; especially noteworthy was the way in which it used hitherto unobserved coincidences in Greek and Armenian, (the correspondence) of the *spiritus lenis* to Armenian *g*, and of the *spiritus asper* to Armenian *v*, to distinguish two different *w*’s in the parent language. All his contributions to the *Classical Review*, and other learned publications, showed the same acuteness of vision and freshness of treatment....

“He was an excellent teacher ; and it was a matter of some regret when he left us for the Bar, though there is no question that his acumen and subtlety admirably qualified him for that profession.

“Mr Darbishire, as all his friends can testify, was a man of a singularly modest and amiable character. His loss makes us sadly feel, in the words of Horace,

‘neque candidiores
terra tulit, neque quis me sit devinctior alter’.”

I add the tribute to his memory paid by Dr Peile, Master of Christ’s, who, in his valedictory address as Vice-Chancellor,

spoke as follows in closing the record of the death-roll of the University during the past academical year :—

Last, aged but thirty years, died Herbert Darbishire, Fellow of St John's, in whom remarkable acumen and ripe judgment were combined with a sweetness of nature which will long be remembered by those who knew him well :—

ὃν οἱ θεοὶ φιλοῦσιν ἀποθνήσκει νέος.

Mr Darbishire won the affection and admiration of his many friends by the singular beauty of his character, and also by the unwavering courage and the perfect good temper with which he struggled against physical weakness resulting from an accident which befell him in early life. The brightness of his intellect, as well as the dignity of his bearing, and the charming and unaffected courtesy of his manner, will long be remembered by all who knew him. As his College Tutor I naturally saw much of him during his residence as an undergraduate. In freshness and originality, as well as in cheerful devotion to duty, he was one of the most interesting pupils I have ever had. He was also one of a small number of students who came from time to time to my house to read standard German works connected with classical literature, works such as Goethe's *Iphigenie* and Lessing's *Laokoon*. In the group of classical students and others who were thus drawn together by a common interest, his geniality, his perspicacity and his acumen were constantly apparent.

In the choice of his friends he was far from restricting himself to those who were interested in the same department of study as himself. Of those who knew him best in his own College, two at least were distinguished in Mathematics and in Natural Sciences. One of them, Mr F. F. Blackman, 'first met him at the whist-table, where he was a keen and brilliant player. Attracted to him by the sparkling yet kindly wit, lodged in a frame that would have made a cynic of a weaker mind, I discovered, as an intimate friend, the real beauty and fineness of his character.' Another, Mr R. A. Sampson, notices two points as chiefly characteristic of his intellectual ability. The first was a singular 'ingenuity, that showed itself in his

work, his amusements,—chess, puzzles, and so forth, and continually in his conversation.’ The second was his ‘independence; so strong a feature as to make it very difficult for his closest friends to do him any service.’ One of his classical friends, the Rev. A. L. Brown, of Trinity and of Selwyn, writes:—‘I knew him at Cambridge, and away; the brightest spot in my memory of him is a visit paid a year ago in his own home. I never knew him below his best. One thing always struck me very forcibly about him; and that was how he absolutely triumphed over his physical infirmity; there never seemed to me to be any signs of a struggle or even any consciousness of its existence. And, moreover, his physical courage was considerable. I have been long walks with him, and I never knew him allow that he was tired, although in going up hill his lungs clearly gave him trouble. For his many-sided intellectual activity it was impossible to feel anything less than reverence.’ Another of his friends, Mr H. J. Spenser, of St John’s, thus sums up the impression left upon him by eight years of unbroken intimacy:—“To an intellect which was singularly keen and penetrating, he united a breadth of mind and generosity of thought which were unbounded, and an intuitive perception of and consideration for the feelings of others, which won the hearts of all with whom he came in contact. The lesson of his life has not been lost. A friend, writing to his parents, assured them that ‘his life, though short, had not been lived in vain’.”

A mural brass in the Ante-Chapel of St John’s bears the following inscription:—

IN MEMORY OF THE
 GENEROUS NATURE AND
 GREAT PHILOLOGICAL GIFTS OF
 HERBERT DUKINFIELD DARBISHIRE
 FELLOW OF THE COLLEGE
 BORN AT BELFAST, 18TH MAY, 1863;
 DIED IN COLLEGE, 18TH JULY, 1893.

After the funeral service in the College Chapel on July 20, a hope was expressed on the part of friends from other Colleges that it might prove possible to arrange for the publication of Mr Darbishire's philological papers in a collective form. His books and manuscripts were readily placed in my hands by members of his family, and were carefully examined with the aid of Mr R. A. Neil, Fellow of Pembroke, and Mr R. S. Conway, Fellow of Gonville and Caius College, and now Professor of Latin at Cardiff. Early in 1894 the Philological Society of Cambridge granted 'a sum not exceeding £30 for the purpose of printing and publishing the unpublished philological manuscripts of the late Mr H. D. Darbishire in a memorial volume, it being understood that each member of the Society receive a copy of the volume.' It also authorised 'the republication in the same volume of the late Mr Darbishire's papers already published by the Society.' A similar sum was obtained partly by subscriptions contributed by some of his personal friends, and partly by the sale, under his father's sanction, of such of his books as had not been presented to the University Library or to that of his own College. Naturally, there were only a few that were not already comprised in the former; but the latter received as many as 160 accessions, about two-thirds of them being on subjects connected with Comparative Philology, and the remainder consisting of editions of Greek and Latin authors and books of reference. All the volumes thus presented by his father bear a special book-plate:—

E LIBRIS

HERBERTI DUKINFIELD DARBISHIRE

COLLEGII DIVI IOANNIS SOCII

QUI TRIGINTA NATUS ANNOS OBIIT A. S. MDCCCXCIII.

FILII DESIDERATISSIMI IN MEMORIAM

PATER EIUS DONO DEDIT.

The grant promised by the Cambridge Philological Society, and the funds derived from other sources, appeared to warrant

an application being made to the Syndics of the University Press with a view to their publication of the proposed memorial volume. To this application the Syndics readily acceded in June 1894, and, since that date, they have liberally undertaken all the additional expenses of printing and publication beyond those originally contemplated. The thanks of all who are interested in perpetuating the memory of the author of this volume are due primarily to the Syndics for their liberality as publishers; also to Messrs Macmillan and Mr Nutt for their kind permission to reprint certain contributions to the *Journal of Philology* and the *Classical Review* respectively; and especially to Professor Conway for the care and skill, as well as the loyal devotion, with which he has discharged his duties as editor.

J. E. SANDYS.

CAMBRIDGE, *June*, 1895.

EDITOR'S PREFACE.

FEW tasks could be at once more mournful or more interesting than to edit such writings as those left behind by my friend Mr H. D. Darbishire. I gladly acceded to the request of the Cambridge Philological Society to put together in a memorial volume the Essays which he had published since 1888, with the scarcely less numerous papers which remained in various stages of completion at his death. This is hardly the place for words of private grief; but I cannot refrain from expressing my sense of the loss which learning has suffered in the premature curtailment of Mr Darbishire's work. His keen intellect and brilliant powers of exposition, well known to all who knew him, will be manifest to the readers of even this brief volume; but those who have followed the rather tortuous development of Comparative Philology in recent years will realise even more profoundly how rare a contribution to knowledge it was in his power to make. I should like, in particular, to direct attention to the Essays in the Theory of Philology (pp. 149—198), which, short as they are, appear to me to place some of the fundamental principles of the Science in a clearer light and on a stronger basis than has been done by any other writer.

In selecting the studies here presented from the printed and unprinted matter at my disposal, as well as in settling their final form in points of detail, I have been guided simply by the principle of publishing no more and no less than what, judging from a long and fairly intimate knowledge of Mr Darbishire's methods of work, I believed he would himself have wished to

appear in book form. In Part I all that was needed was to add references to connected points in the Essays of Part II, and to discard the less important parts of various reviews¹; but in dealing with the papers in manuscript there was more room for mistake, because of Mr Darbishire's characteristic habit of writing down in full new ideas, and even of completing connected paragraphs, as they occurred to him at the moment. Here I have used my own judgment as carefully as I could, preferring always to err on the safe side, that of suppression. Within the body of the text there are none but the slightest alterations of wording, for Mr Darbishire's first written draft was as lucid as most men's first proof. Such additions as seemed necessary in editing are everywhere enclosed in heavy square brackets (except the title-pages to the different sections, for which I am alone responsible); all footnotes and parentheses, not thus distinguished, are the author's.

The preservation of the papers and the inception of this volume are due in the first instance to the care of Dr Sandys. I have also to acknowledge valuable help from two friends and former pupils. Mr F. W. Thomas, M.A., Fellow of Trinity, and now Classical Master at King Edward's School, Birmingham, most kindly offered to read through the Essay on the Sanskrit Liquids, and add any necessary references, so that the text of this paper has the additional security not only of his wide Oriental scholarship but of his special interest in all questions relating to the Indo-European liquids and sonants, though it will of course be understood that neither he nor I desire to pledge ourselves to the details of Mr Darbishire's fascinating theory. Miss Eleanor Purdie, Marion Kennedy Student of Newnham College, Cambridge, verified all the references in

¹ The following are altogether omitted :

Notice of Strong's *Translation of Paul's 'Principien,'* Class. Rev. 1891, p. 387.

Scrumpuli (notes on I.-Eu. negatives), *ibid.* p. 485, and p. 194 in the following volume.

Review of Flügel's *German Dictionary*, Athenæum, Jan. 14, 1893, and 'about thirty odd columns of brief notes on school books or contributions to philology.' (Ed. Athenæum, writing Oct. 2, 1894, for whose kind help my best thanks are due.)

the Essays in the Theory of Philology, supplied others that were wanting, and gave me considerable help in editing the manuscript of the last two papers in this section which we found in great confusion.

The thanks of reader and editor alike are due to the liberality of the Syndics of the University Press in extending the limits originally planned for the volume; to Dr Sandys and Mr R. A. Neil for their kind help in reading the proofs; and to the unfailing care of the press reader.

R. S. CONWAY.

CARDIFF, *June*, 1895.

PART I. PAPERS ALREADY PUBLISHED.

1. ORIGINAL ESSAYS AND NOTES.
2. SELECTIONS FROM OCCASIONAL WRITINGS.

1. ORIGINAL ESSAYS AND NOTES already published.

- (1) The *Numasios* Inscription (1888).
- (2) On the text of Tac. *Ann.* I. 32 (1889).
- (3) Notes on the Spiritus Asper (1889) with Addenda (1890).
- (4) Lat. *omentum* (1890).
- (5) On the meaning and use of ἐπιδέξιος, ἐπιδέξια: ἐνδέξιος, ἐνδέξια (1890).
- (6) On the Indo-European words for Fox and Wolf (1892).
- (7) On the form κατασβῶσαι Herodas v. 39 (1892).
- (8) Some Latin etymologies (*altus, colo, iubar, numen, scio*) (1893).

(1) THE *NUMASIOS* INSCRIPTION

(published in the *Journal of Philology* xvi (1888) p. 196).

THE NUMASIOS INSCRIPTION.

TOWARDS the end of last year a tomb was opened at Praeneste by Helbig and Dümmler, in which was discovered a gold fibula bearing a very interesting inscription—the interest due to the form being considerably increased by its being supposed to date from before 509 B.C., which makes it much older than any other Latin inscription.

An abstract of the paper which Dümmler read before the ‘archäologisches Institut in Rom’ appeared in the *Wochenschr. für klass. Phil.* of Jan. 26th, 1887 (No. 4, col. 121). The inscription is briefly noticed in Wölfflin’s *Archiv für Lat. Lex.* 1887, Pt. I p. 143. More important than these however is a paper by Bücheler in the *Rheinisches Museum* (Vol. XLII p. 317), and it is the latter which forms the immediate occasion of the present article.

The inscription as given by Bücheler runs:

|o|sA W W W W : d x A | s t : s t : d s W W : s o | V A W W

manios · med · vhe · vhaked · numasioi

After the second *vh* were visible traces of an upright line, apparently a mistake corrected.

In discussing such an inscription the three heads to be taken up are, palaeography, phonology, and morphology, inasmuch as the *matter* is unimportant.

1. Palaeography.

a. The direction of the writing is *retrograde*. Hitherto the canon had been (e.g. Hübner, *Müller’s Handbuch*, Vol. I¹ p. 496) that this order was only used in *devotiones* when the meaning was to be intentionally obscured.

b. The form of the letters,

If any one will compare the characters used here with those on the Dvenos inscr., which stands next in antiquity, he cannot fail to be struck by the differences they present, particularly in the signs for *d*, *h*, *o* and *s*. Again, a glance at the Greek inscriptions on western soil, either directly in Roehl (507—550) or by means of the tabulated results in Kirchhoff or Hinrichs (*Handb.* l. c. p. 416), will suffice to show that the similarity of their alphabets to this is very marked indeed and becomes more so in the Italian examples, so much so that we are fairly justified in describing this as a Greek alphabet, borrowed but not naturalised. The importance of this result will appear below.

c. The words are divided by double points instead of a single one as common later. In '*vhevvhaked*' the first syllable is divided from the rest by ∴. This seems merely to bear out the evidence for the hystero-gene nature of the form. Bücheler's parallel *im · perator*, with *tribarakat · tins*¹ (*Cipp. Abell.*), *medicat · inom* (*Tab. Bant.*) and *καταλείπον ∴ τα*² [*I. G. A.* 321], are only additional evidence that the constant element of the word was kept mentally distinct from the variable terminations and prefixes.

2. Phonology.

a. The combination '*vh*'.

This is the transliteration which Bücheler suggests, meaning presumably Latin *v*, not German or English. In this I follow him, but justify the transliteration on very different grounds.

His view is, in brief, that this collocation represents a stage in the process by which the voiced aspirates *dh*, *bh*, *gh* passed in Latin into *f*. Now a principle of the utmost importance for directing the science of language is this: that no phonological change is to be considered as fully established until each step has been shown to be *natural* on phonetic grounds³. It is the recognition of this which forms one of the merits of the modern school; it is this which decides the superiority of 'sonant

¹ Bartholomae (*Bezz. Beitr.* xii 82) denies that this has any phonetic value.

² This has also been explained as a mistake.

³ [A fuller consideration of this point will be found in the paper on *Phonetics and Philology* in Part II. C.]

nasals' over 'nasal insertion' as an explanation of the same set of phenomena.

This canon Bücheler violates when he speaks of *dh*, *bh* and *gh* taking a parasitic *v* between the sonant and the aspirate, and when he goes on to say that from the *dvh*, &c. thus resulting, *either d*, &c. fell off, leaving *vh* (of which this is an instance) which passed into *f*, or *dv*, &c. fell off, leaving *h* only. Even allowing the possibility of a parasitic *v* after a *velar* guttural, which is phonetically explicable, the combination would be *ghv* and not *gvh*, while for the palatal, dental, and labial series such a hypothesis is entirely unjustifiable.

It may be urged that as long as a phonetic explanation of the change of the sonant aspirates to *f*, &c. is wanting, the insertion of hypothetical stages is permissible. It will therefore be scarcely out of place, if I attempt here an explanation of the change on phonetic lines.

A sonant aspirate contains in itself two antagonistic elements. While the vocal chords are stretched the current from the lungs is more or less impeded and softened. On the other hand the expulsion of a strong breath tends to force the chords apart and leave the passage free. Hence a sonant aspirate is an unstable combination and practically unknown in Europe except in the Irish pronunciation of English.

When this unstable sound is modified it must be done in one of two main directions, (1) in favour of the sonant, or (2) in favour of the aspiration. The latter again has two subdivisions, (*a*) the sonant may become its corresponding surd, (*b*) the aspiration may prevail entirely and produce a sound which may be called *h* if we remember that *h* is wholly indeterminate. The only other modification worth mentioning is that adopted by Teutonic in which the concession to the aspirate is made, not in the larynx, but in the mouth, resulting in *j*, *3*, *ä*, *w*¹ respectively. Of these methods (2 *a*) is best exemplified by ancient Greek. Latin adopted (1) in the middle of a word where a strong breath is in any case difficult. Hence the rule

¹ The value of these signs is that given by Sievers, *Phonetik*, p. 127. This method of reduction does not seem to have been employed in Latin unless the *v* in *nivis* is an attempt to represent *3*.

for Latin that *gh*, *dh* and *bh* medial pass into *g*, *d* and *b* respectively. On the other hand, at the beginning of a word the lungs are full and a strong expiration is easy; here then (2 *b*) is the method adopted. But the indeterminate *h* resulting therefrom, although indeterminate in writing, is by no means so in speech, its value being fixed by the following considerations¹. If the sound which it replaces was *gh* the only tendency to contact will be at the back of the mouth. Hence the breath will have a slight guttural character and will be accurately represented by *h*. On the other hand if the sound replaced was *dh* or *bh*, the tendency to contact is at the front of the mouth, the lips are approached and a bilabial sound is formed which bears most resemblance to *f*. Hence we may formulate the rule that *gh* initial, in Latin, passes to *h*², but *bh* and *dh* become *f*. Further, the *f* which comes from *bh* will be purely a bilabial sound, and a very slight severance of the lips would render it indistinguishable from *h*, whereas the *f* from *dh* would naturally be more labiodental in character and so be stable. The table given by Stolz, § 58 (*Handb.* II¹ p. 177), should therefore be modified as follows for initial sounds:

Indo-Eur.	<i>gh</i>	<i>dh</i>	<i>bh</i>
Lat.	<i>h</i>	<i>f</i>	<i>f</i> <i>h</i>

If this account of the process is reasonable, the necessity for assuming a parasitic *v* falls to the ground, while a direct argument against Bücheler's view is supplied by the rule that *dh* medial passes into *d*. The sequence he seems to assume is I. Eur. **dhēdhē*, Latin **dvhēdvhē*, then **vhēvhē*, but I. Eur. **dhēdhē* would give Latin **fēdē* (Osth. *Perf.* p. 207).

I believe the explanation of this orthography to be given at once by the nature of the alphabet, which was shown above to be *Greek*. That being so we have here to deal with a *transliteration*. But in a Greek alphabet of this date, no sign for *f* existed and the engraver would find some difficulty in representing the sound. The nearest equivalent would obviously be a breathed

¹ [Cf. p. 21 inf. C.]

borrowed. Osthoff, M. U. 4. 99. Brgm.

² Cases of 'f' are to be regarded as *Grundriss*, § 389.

digamma and as such I regard this combination. To mark that a sound is to be breathed, *h* is used as a diacritic by the Greeks themselves, e.g. ρ^h = breathed ρ (Brgm. l. c. § 266) and Fh as here (I. G. A. 131). This breathed **F** would differ very slightly from bilabial *f* and its regularly passing into *h* in Greek (Brgm. l. c. § 166) supports what was advanced above on the second change of *f* from *bh*¹. That no distinction was made in writing, between *f* bilabial and *f* labiodental, is not to be wondered at. Of course when the alphabet had been naturalized and *v* was the equivalent for **F**, a double sign for *f* was cumbrous and unnecessary², and Bücheler, with great probability, suggests that while Latin retained **F**, the Umbro-Samnite alphabet simplified in the other direction and Ξ (modified in Umbrian into Θ for *h*) was retained both in Oscan and Umbrian as **8** for *f*.

The vocalism of *fefaked* depends too closely on the morphology to be discussed here.

b. The proper name *Numasius*.

The difficulties presented by this word are not inconsiderable. In classical Latin we have the two forms Numisius and Numerius. Is this connected with either of them or with both?

Of course it is possible to call in the aid of that *deus ex machina*, the anaptyctic vowel. It is also possible with Bücheler to regard *-asius*, *-isius*, *-esius* as unimportant variations of suffix. Perhaps however a more legitimate explanation may be found.

Roman proper names are formed from a comparatively small number of simple roots. These may or may not have a meaning that we can trace—more frequently the latter is the case, for the very fact of a word becoming a proper name implies that it ceases to be used with a connotation. To these simple roots are added a great variety of suffixes both primary and secondary.

¹ This confusion of bilabial *f* with *h* is also illustrated by CIL i. 1501, for Jordan's explanation (*Krit. Beitr.* p. 50 *sqq.*) is hardly plausible.

² An interesting confirmation of this is yielded by a close inspection of the inscription. If Bücheler's copy is accurate, the engraver had actually

omitted the diacritic after the second **F**, and only perceived it after beginning the κ . Thereupon he turned the Δ into Ξ and erased the upright line which was to have formed the κ . This shows that the *h* was felt to be necessary but cumbrous.

An example of a root with traceable meaning is '*manus*' 'a good man', from which come *Manius Manilius Maneius*, &c.; of an obscure root is *Titus*, forming *Titius Titinius Titidius Titurius*, &c. One such old root-name was *Numa*—probably the oldest masculine *a*-stem in the language—which *may* be connected with *νόμος*. With the suffix *-so* (see Pauli, *Altit. Stud.* I 53) this gives *Numāso*- secondary *Numāsio*-, with which the later *Audasio*- *Equasio*- and the like cannot be compared on account of the doubtful quantity of their *-a*.

The chief forms of the names are

A. Latin.

1. Numasius.
2. Numsius Momms. *U. I. Diall.* p. 252. Corss. *Etr.* II 14.
3. Numpsius Momms. l. c. p. 197. CIL I 1211.
4. Numisius classical and frequent.
5. Numaiirius Numiirius CIL IV 2313.
6. Numerius classical.

B. Oscan.

1. *Νῠμσδιης* Zv. 160.
2. *niumsis* Zv. 57.
3. *niunerio* Zv. 24 (late and Latinizing).

C. Etruscan.

Numsi Corss. l. c.

The antiquity of this inscription (as well as the regularity of the formation itself) would lead us to assume *Numasius* as the original for all these forms. In Oscan and Etruscan the short vowel was syncopated as usual, and in this form, if we may trust A. 2 and 3, the name was borrowed by Latin. Another of the names derived from *Numa* was *Numitor*, in which *i* is regular, being in an unaccented syllable before a dental (Stolz¹, § 23, no. 4). It is then not surprising if *Numasius* and *Numsius* give way to *Numisius*. The name in this form is found in classical times, and, with other names in *-sius*, has been much discussed. Jordan (*Krit. Beitr.* p. 104 foll.) concludes that the tradition which makes Roman names in *-rius*

rest upon older forms in *-sius* is unfounded, and that all names in *-sius* are to be considered as borrowed. The prevailing view however is, that the tradition is correct, but that names in *-sius*, in classical times, are to be explained as Jordan does. I scarcely think enough allowance is made for the natural *stability* of spelling in the case of proper names (cf. Le Maistre in modern French), which moreover are specially subject to family caprice, tradition and even politics, as in the case of *Clodius*. It is not to be wondered at then that a certain family should retain *Numisius* although for the most part the rule of rhotacism produced *Numirius*. The subsequent change of *i* to *e* before *r* is regular (Brgm. *Grds.* § 33. 1).

Except for the explanation of the other forms, however, there is no need to insert any stages between *Numasius* and *Numerius*, for *Numarius* would of necessity receive *e* from *numerus*—Quintus Sextus, &c. and Quinctius Sextius, &c. giving the proportion both for praenomen and for nomen. See also Varro *ap. Non.* 352. 29 *qui celeriter erant nati fere Numerios praenominabant.*

Numisius gives in all probability the suffix *-sius* which forms names in *-irius* (e.g. *Papirius* from *Papius*¹), to analogy with which may be due *Numiirius* in A. 5. In the case of *Apisius* and the like, the quantity of the *i* depends on whether they are formed from *Apus* or *Apius* (*Appius*).

One name seems to have followed step by step the development of *Numerius*. This is *Valerius*, which has *Vala*, also an *a*-stem, as base. That *Numerius* set the analogy, is certain from its superior antiquity, as shown by its twofold use. If the above considerations have any weight we must correct the statement of Festus (p. 23 Müll.) that the older form of *Valerius* was *Valesius*. It should be *Valisius* after *Numisius*, which would become *Valerius* as above. The existence of a suffix *-sius* is questionable, for, in the examples quotable from the Corpus, both the quantity of the *-e*-, and its forming part of the suffix, are doubtful.

¹ Jordan l. c. seems to make *Numisius* stand to *Nummius* as *Papesius* does to *Papius*, which is impossible from the difference of quantity.

3. Morphology.

a. The character of the language.

This I have assumed to be Latin, on the authority of Bücheler, whose argument is that it lies between Latin and Oscan and is shown not to belong to the latter by the presence of *med* for which, to judge by the analogy of *siom* = Latin *sē*, Oscan would have '*miom*.'

b. '*med*.'

This accusative with the 'ablative' ending *-d* shows that the confusion we find in Plautus is as old [at least] as Latin of the sixth century B.C.

c. '*fefaked*.'

That this cannot represent an original reduplicated form of the root *dhē-* was shown above, by the consideration that **dhēdhē* would in Latin become **fēdē*, while the interpunctuation points to the same conclusion. From the cognate forms the following may be suggested as a possible history: $\sqrt{dhē}$ Perf. 1st sg. **dhēdhē-a* = **dhēdhē* (Sanskrit *dadhā-u*, Greek *τέθη-κα*); as in Greek, this was probably strengthened by the deictic particle *-ke* or *-ka* (the later *-ce*) either as an independent developement or inherited by each from the common stock¹. The case for Greek is stated in Osthoff, *Perf.* p. 326 f. As in Greek also the *k* spread into the other tense systems, as the aorist *dhēk* = *θηκ*² whence the classical perfect *fēc-i*, and present *dhāk-* with the short vowel originally in plural only, but extended into the singular. This may be considered as pre-dialectic, for it is found in Oscan *fe-fac-ust* *fe-fac-id* (with hysterogene reduplication as here) *fakurent*, &c., in Umbrian, in classical Latin as in *facio*, and in this form. Bücheler thinks that the erased line points to a form *feficit* analogous to *tetigit* which the engraver was going to write but changed his mind. Perhaps the erasure is better explained as above. (Note 2, p. 10.)

The ending *-ed*, its character, and the quantity of the *e*,

¹ [I hardly think Mr Darbishire retained this view after the appearance of Per Persson's *Wurzelerweiterung*

(p. 209 f.). C.]

² This equation is Bartholomae's (K. Z. xxvii. 255).

have been fully discussed by Osthoff (*Perf.* pp. 205—232) whose conclusion is that this “3. sing. auf *-ēd*” is to be regarded as an “alte ur- und gemein-italische Neubildung zu der 3. plur.” This therefore brings nothing new to bear upon his result.

d. Numasioi.

The chief direct testimony to the use of this dative in *-oi* in old Latin, corresponding to the Greek *-φ*, had hitherto been that of Marius Victorinus (G. L. 17. 20), and it had been considered doubtful by some, e.g. Jordan, *Krit. Beitr.* p. 241.

The collateral evidence however, the analogy of Greek, and of Oscan forms like *Núvlanúí*, was very strong at least for its existence in Italy. This insc. however proves it to have lasted into Latin.

The general results of the discovery may be summed up as follows. It supplies a step in the history of alphabets, as it shows the process of borrowing from Greek in actual operation and as yet incomplete. It throws the separation of dialects in Italy to a very early date, as Latin is not only separated, but is considerably advanced towards its classical stage—of course this is assuming the accuracy of the ascribed date. It gives evidence of the existence of a single-name epoch and shows that that name might be a patronymic, thus explaining the existence of such among *praenomina*. Its bearing on other inscriptions is naturally unimportant from both its brevity and its antiquity, but it has a slight connection with the interpretation of the ‘Dvenos.’ Pauli (*Altit. Stud.* i 1 ff.) makes two assumptions—that the retrograde order was *not* primarily due to desire of secrecy, but was a relic of an older custom,—and that the dative masculine in Latin ended in *-oi*. These are no longer assumptions but are actually in evidence, and to this extent the discovery supports his view.

(2) ON THE TEXT OF TACITUS, *ANNALS* I. 32

(*Rheinisches Museum* 44 (1889) p. 319).

ZU TACITUS, *ANNALEN* I. 32.

NIEMAND meines Wissens hat bisher diese merkwürdige Stelle angetastet. Die allgemeine Lesart: *repente lymphati dstrictis gladiis in centuriones invadunt: ea vetustissima militibus odiis materies et saeviendi principium. prostratos verberibus mulcant, sexageni singulos, ut numerum centurionum adaequarent. tum convulsos laniatosque...* ist schwerlich richtig. Dass je sechzig Männer die einzelnen Centurionen durchprügeln, entbehrt aller Wahrscheinlichkeit. Alle zusammen wäre ja unmöglich: nach einander sieht für meuterische Soldaten allzu langweilig aus. Dass diese in der Strafe die Anzahl der Centurionen gelten liessen, ist verstehbar und selbst natürlich, nicht aber jene Art der Geltung. Die Verbesserung liegt nahe. Man denke nur, wie leicht von *singulos* ein *s* wegfiel und lese *sexagenis*. Die Zahl der Hiebe gleicht der der Centurionen: sie genügt freilich *laniatos* zu rechtfertigen.

(3) NOTES ON THE SPIRITUS ASPER
WITH ADDENDA

(published in 1889 in the *Cambridge Philological Transactions*
Vol. III p. 77 and in 1890 ib. p. 119).

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NOTES ON THE SPIRITUS ASPER.

§ 1. THE sound which we are accustomed in English to denote by the letter *h* (initial) occupies the singular position of being to all appearances the independent development of each Indo-European language in which it is found. From this it naturally follows that one ought not to expect to find it phonetically identical in all, and accordingly the first duty of any one who touches upon it is to state what he believes to be its nature in the particular language under discussion.

The evidence on which rests the conclusion that the original I. Eu. language did not possess any distinction of vowel opening, or that if so it was subsequently obscured, is that in Vedic all words with initial *h* come, as may be seen at once from Grassmann, from older *gh*, while in Greek the spiritus asper appears to come almost universally from a similar change affecting other sounds. The exceptions for Greek (if any) will appear in the course of this paper, but are not at all events sufficient to warrant any assumption of original *h*.

Sweet (*Handb. of Phonetics* § 195) makes our English aspiration the 'gradual' beginning with the glide stressed. It therefore follows that its exact phonetic value depends on the vowel which comes after it. Quite different is the German *h*, which has more claim to independence. According to Czermak¹ the common characteristic of all true *h*-sounds is that they are *Reibungsgeräusche*, produced by a narrowing of the channel traversed by the breath-current. The English *h*, also, appears to me to have this character in many cases,

¹ *Ges. Schriften* 1. p. 755.

which may account for the disagreement between Sweet and Ellis¹. What we know of the origin to be assigned to the *h*-sounds in Greek and Latin (and probably in Sanskrit) points to the supposition that these also were of this description.

It is obvious that the 'narrowing' (*Enge*) of the channel, above mentioned, may take place at any point between the larynx and the lips, and just as we have (1) Explosives, (2) Fricatives, so we can have (3) Aspirations, guttural, palatal, dental, labial, and so on accordingly, the extremes being German *h* and English (northern) *wh*, which is strangely considered a double sound by some phonetists. In other words we have for every point the following table:—

<i>Condition.</i>	<i>Result.</i>
Total contact.	Explosive.
Close approximation.	Fricative.
Narrowing.	Aspiration.
No resistance	Deaspiration.

Thus when the initial group σF in Greek passes first into F' and then into the spiritus asper, there is a fair probability that with the loss of the σ the F becomes breathed² and then passes into the labial *h*-sound. Similarly we may assume that the spiritus asper from σ is dental, and that from ξ palatal in character. These remarks it may be observed apply also to the history of the Latin aspiration: we may assume a labial character for the *h* from *bh*, a guttural character for the *h* from *gh*. So also for Sanskrit.

To explain why the languages mentioned did not distinguish between these sounds, it must be noted that their audible value differs very slightly. That is to say, the speaker may be consciously forming a labial *h*-sound without the hearer's being able to distinguish it from the sound he himself forms at the back of the mouth. Of course the reminder is hardly necessary that speech is learnt by the ear alone. Con-

¹ Sweet, *Handb.* p. 65, note *a*.

² or *fh* as in I. G. A. 131.

³ More correctly σf - by mutual assimilation became $f'f'$ - which is writ-

ten *f*- just as $\sigma\mu$ - became $\mu'\mu'$ written μ (or μh as in I. G. A. 344). [See further Excursus I. p. 54 inf. *C*.]

sequently there is no need to suppose that, say at 400 B.C., any Greek made, or was conscious of, a distinction between the aspirations of *ὄς* and of *ῥδύς*. That the sound tended to become less and less definite in character is shown (1) by its misplacement, (2) by its gradual disappearance.

The object of this paper is to investigate whether its etymological value is—subject to the usual limitations—exact and definite, or the exceptions to the rules too numerous for it to have any weight in confirming or rejecting a derivation.

§ 2. The easiest way to disclose the exceptions is to begin by giving the usual rules with the most certain examples of each.

I. Original *s-* becomes '*i*' in Greek¹.

EXAMPLES:—

αἶμα Germ. *Seim* √ *si* Fick, Vol. I. 799, more probable than Skt. *asan* Christ, *Lautl.* p. 139.

αἶμων Skt. *sev-* L. Meyer, p. 89.

ἄλλομαι = I. Eu. *slīō* Lat. *salio*.

ἄλς, *ἄλας* Lat. *sal*.—*ἄλς* may equally well be from √ *suēl*, Lat. *salum*.

ἄρπαξ &c. Lat. *sarpo*, which is more directly parallel than *rapio* (see *infra*). Here probably belong *ἄρπεξα* 'a thorn-hedge' and *ἄρπεδόνη* 'a noose'.

ἔδος (*ἔζομαι*, *ἰδρύνω* &c.) √ *sed*.

εἷς (*ὁ-όμο-*) √ *sem*.

ἔλος Skt. *saras*.

ἔξω √ *segh*.

έός = I. Eu. *seuos*.

ἐπομαι √ *seq*.

ἐπτά = I. Eu. *septm* (*septm̃*, Wheeler *Nominalacc.* p. 19, less probable).

ἔρμα √ *ser* 'link' or 'connect'². Latin *sero sertum ἔρμος ἐρμηνεύς* &c. To this sense are easily traceable the different mean-

¹ Brgm. *Grds.* § 564.

² Perhaps identical with √ *ser* = 'move' and so connect *ὁρμή* &c. 'Ἐρμή' perhaps 'the connector' but cf.

L. Meyer p. 693, Wackernagel K. Z. 25, 267 who refer it to *Sārameyā-* and so to *sāra* = ἤρω (L. Meyer p. 694).

ings of ἔρμα, including that in *Il.* 4. 117, where ἔρμ' ὀδυνάων is not the 'foundation' of pangs (so Liddell and Scott), but the 'link' which brings them.

ἔρπω Lat. *serpo*. *rēpo* seems to stand to *serpo* in the same relation as *rapio* to *sarpo*.

εὔδω Lat. *sudum*. Fick, Vol. II. p. 259. Highly improbable.

ἡμι- Skt. *sāmi*- Lat. *sēmi*-.

ἱ- acc. ἱν, instr. ἱνα. Vedic *sīm* which cannot be referred to *sa*-.

ἱημι = I. Eu. *si-sē-mi*.

ἱμάς √ *sī* more probable than *vī*- (*vīmen* &c.).

ἱστημι = I. Eu. *si-sthā-mi*.

ὁ ἦ = *sas sā* with its cognates.

ὀδός Skt. *sādā*-.

ὅλος Skt. *sarvas*, Lat. *sollus*.

νίος = √ *su*.

ὥραξ Lat. *sorex* perhaps √ *suer*.

ὤς Lat. *sus*.

ὕλη Lat. *silva* √ *suel*.

ὕπνος Skt. *svapna*-, Lat. *somnus* √ *suep*.

II. Original *j*- becomes *ʿ* in Greek¹.

EXAMPLES:—

ἄζομαι; ἄγιος, ἄγνός cf. Skt. *yaj*.

ἡπαρ = Skt. *yakṛt*.

ὤς = Skt. *yas*, and cognates ἔως (= *yāvat*²) ὡς &c.

ὕμεις compare Skt. *yuṣma*-.

ὕσμινη √ *j*udh.

ῥα Gothic *jēr*.

III. Original *su*- becomes *ʿ* in Greek³.

EXAMPLES:—

ἀδύς ἀνδάνω &c. Latin *suavis* √ *suād*.

ἔ Skt. *sva*-.

ἐκυρός Skt. *svācura*-.

¹ Brgm. *Grds.* § 129.

³ Brgm. *Grds.* § 564, cf. p. 149.

² Not directly parallel.

ξξ = s̥eks.

ἥλιος Skt. *sūrya-* √ s̥yer.

ιδρώς Skt. *svid-*.

IV. Original s̥i- becomes ' in Greek¹.

ὑμὴν ὕμνος √ s̥ia^zu Skt. *syūman-*. I make ἀδρός = s̥ind-ró- (Ved. *syand-* 'flow'), cf. Hdt. 4. 31, Arist. *Probl.* 28. 1, p. 949^b 5.

V. Original u- becomes ú- in Greek².

Except in dialectal forms, as Lesbian, ú is not found in Gk. In many words the aspiration is etymologically justified by one of the above rules, but in many others the aspiration can be proved hysterogene, e.g. ὕδωρ cf. *unda* weak form of √ u^ad-, ὑπέρ ὑπό Skt. *upari upa*, ὕστερος Skt. *uttara-*, ὕπαρ cf. Skt. *vapus-* (L. Meyer, p. 945).

The only explanation offered of this phenomenon will be found in Brugmann's *Grundriss* I. § 48, and is as follows:—that *u-* developed a parasitic *ǵ-* which passed into the spiritus asper in accordance with our 2nd rule. The proximity of *ǵu-* and *ũ* is vouched for by the English pronunciation of *u* as in *use* (which is generally supposed to have originated in an attempt to pronounce the French *u*) and by Boeotian *ιου* for *υ* as in *τιούχα* (e.g. Cauer² 303). The case is by no means established. Boeotian *ιου* proves nothing for it only occurs after dentals (τ, δ, θ, λ, ν), and hence like the similar Oscan phenomenon³ is to be ascribed to dentalism. The parallel drawn from English is obviously inexact and that in two ways: first, the assigned explanation implies that at a time when the Greeks naturally said *u* they made attempts at saying *ũ*, but at first only succeeded in saying *ǵu*, whereas the change of *u* to *ũ* in Greek was purely phonetic and therefore unconscious: secondly, Boeotian (with *u*) is to the rest of Greece (with *ũ*) as English (with *u*) is to French (with *ũ*), therefore the initial *ǵ* and consequent aspiration should appear in Boeotian and be wanting elsewhere; which is exactly the reverse of the facts.

¹ Brgm. *Grds.* §§ 564, 131.

² Osth. *Perf.* p. 485.

³ With which Brugm. (l. c.) also compares it.

Further, the theory necessitates one of two conclusions, neither of which has been drawn from it, or is in itself probable. The intrusive $\dot{\alpha}$ - of the hypothesis must have come in either before or after the separation of dialects. If before, the conclusion is that Boeotian violated in this case its regular change of $\dot{\alpha}$ - to spiritus asper¹. If after, i.e. in the 6th century B.C.², it is necessary to suppose that the change of $\dot{\alpha}$ - to spiritus asper took place independently in the several dialects, and moreover, that in some of them it took place *twice*, for the traces of $\dot{\alpha}$ - in the Homeric poems are known to be of the slightest.

I am therefore reluctantly compelled to record the phenomenon as unexplained, nor can I offer any attempt to solve the problem³.

§ 3. Of these five rules, the first three are generally believed to admit of a large amount of exception. Such exceptions obviously are of two kinds: first, words which have not the aspirate, although their derivation brings them under one of the rules: second, words which have the aspirate without coming under the rules.

The first of these again will fall into three subdivisions.

a. Original *s*- becomes ' in Greek.

The evidence for this is most fully given by L. Meyer, pp. 89 sqq., whose instances are:—

ἡΐελιος = ἥλιος

οὔλος = ὄλος

ἀμόθεν, ἀμῆ, &c.: ἀμόθεν, &c.

ἀθρόος = ἀθρόος

ἀνειν : ἀνύειν

ὄρηξ = ὄρηξ

Compounds with $\dot{\alpha}$ - = $\sigma\eta$

ἑτεός = Skt. *satya*-

ἔδεθλον, ἔδαφος: ἔδος

ἄνευ = Skt. *sanutar*

ἐνιοι: $\sqrt{\sigma\eta}$

ἐναλίγκιος: $\sqrt{\sigma\eta}$

εἶρειν = L. *serere*

ὀρός = L. *serum*

¹ As for example in the forms of the relative pronoun (stem $\dot{\iota}\omega$ -).

² The reasons for fixing the pronunciation as *u* in the 6th cent. are two: 1st the Latin transliterations at the early period of Greek contact, 2nd ϕ is found in I. G. A. 524 and others (dated

middle of 6th cent. Roberts, *Introd.* p. 210) before *v*, which proves *u* not *ü*. Besides, the lateness of the aspiration is proved by the non-dissimilation, e.g. in *ἰφῆ*.

³ [See further Excursus II. p. 55 inf. C.]

ἄμεναι, ἄτος, &c.: Lat. *satur*

ἱκμάς : Skt. *sic-*

ἐνεπε = Lat. *in-sec-e*

ὀπάων : Lat. *socius*

ὀπός = Germ. *Saft*

Ἐρινύς = Skt. *Saranyu-*

ἐρύεσθαι = Lat. *servare*

ἄλσος = Lat. *saltus*

ἄλσο, &c. : ἄλλομαι

ἐάω : √ *su* Fest. *desināre*.

Of these, ἥελιος is discussed at length below (p. 37). οὔλος like οὔδος, οὔρος, &c. is purely dialectal. In ἀμόθεν the initial aspirate disappears by the rule for dissimilation, ἀμόθεν : ἀμῆ then produced ἀμόθεν and ἀμῆ. The same holds for ἀθρόος, and the other compounds with ἀ- for á-, e.g. ἄλοχος, &c. produce ἄκοιτις, &c. The same rule produces ἔδεθλον ἔδαφος beside ἔδος. For ἄνειν, ὄρπηξ, ἄμεναι, &c. see pp. 38, 35, 38, respectively. The connexion of ἱκμάς with *sic-* is by no means a certain one. Geldner¹ proposes to connect it with Skt. *yāṣu*, Zend. *īṣi*, but Düntzer's² view, who connects ἱκμαλέος ἱξύς, &c. with Skt. *vij-* Germ. *weich*, is perhaps preferable on account of

Κυκλός, ἄφαρ δέ τε ἱκμάς ἔβη, δύνει δέ τ' ἀλοιφή,—P 392,

which seems to demand the F. The derivation of ἐνεπε from √ *seq* was refuted by Brgm. K. Z. 25. 305 and that from √ *ueq* substituted³.—If it were established that *s-* could vanish the derivation of ὀπάων from *seq-* would be highly probable, but it cannot aid in establishing the rule, because it is equally possible to connect it with ὄπις 'respect', and so with οἶ- 'see'.—The comparison of ὀπός with Germ. *Saft* is fanciful and still more so that by Curtius with σαφής, *sapor*, or *sucus*. I believe⁴ the radical meaning to be 'bitter', but do not identify it with the root *ak* because that involves transition from one class of gutturals to the other.—The derivation of ἐτέος from *satya-* does appear to warrant the rule, and has been frequently adduced to support this and other irregularities. But in fact, except

¹ K. Z. 27. 255.

² *Ib.* 13. 19.

³ ἐνεπε: ἐνεπε as explained by Wackernagel, K. Z. 25. 262.

⁴ [In a marginal note Mr Darbishire says; 'better as Fick⁴ I p. 15', i.e. ὀπός

should be derived from a root *ōp*: *op* which Fick traces in Ved. *apās* (gen. sg.), *āpas* (nom. pl.), Lith. *upė*, Pruss. *ape* 'river' and possibly in Ἀσωπός, Ἰνωπός. C.]

the similar meaning there is no particle of reason for identifying the two. Brugmann¹ and Osthoff² have vainly tried to evade the difficulties. *Satya-* is for I. Eu. *ḡt-īo-* which would give in Greek nothing but *ἄσσό-* which bears no great resemblance to *ἑτερο*. That the latter is a stem in *-τεF-* is proved by Cretan *ἑτεφανδρος*³ (if to be identified with this) but certainly by the weak stem in *ἔτυ-μος*. It is thus parallel to *ποιητέος* and the other so-called 'verbal adjectives'. The difference in accent between *ἑτεός* and *ποιητέος* is a good example of Wheeler's rule⁴, for *ἑτεός* is not dactylic.

The next question is the root: and first as to the digamma. Knös (p. 198) gives the Homeric evidence as follows:—Hiatus before it, B. 300, E. 104, Θ. 423, M. 217, N. 153, 375, Ξ. 125, γ. 122, ι. 529, ν. 328, π. 300, 320, τ. 216, ψ. 36, ω. 259, 352. Elision before it, H. 359, M. 233, O. 53, Σ. 305, Τ. 255, Φ. 107. As he follows the derivation *satya-* he is of course compelled to slight the first instances, and deny the F. The cognates I propose compel me, in the first instance, to make an equally gratuitous assumption that the word has the F, leaving the evidence to be examined later.

ἑτερο-ος is then the verbal adjective of a root *uē*, *uě* (? *u*): of this the present in Greek is *ἑρμι* 'say', 'speak'. This verb is thus given an independent existence and the rather improbable re-formation⁵ from the single form *ἦ* becomes unnecessary: of course *ἦμι* cannot be directly for *ēgh-mi*. The radical meaning of *ἑτεός* is therefore simply 'what is to be said', and hence 'true', just as the Latin *vēr-us*, from the same root, arrives at the same meaning through the suffix *-ro-*.

The light thus thrown on the original meaning of the word renders intelligible II. xiv. 124,

κέκαστο δὲ πάντας Ἀχαιοὺς
ἐγχείη· τὰ δὲ μέλλει ἀκούμεν, εἰ ἑτεόν περ,

¹ Curtius' Stud. 9. 334.

² K. Z. 24. 414.

³ Curtius' Stud. 9. 102, cf. 334.

⁴ Nominalaccent im Griech. pp. 60—104.

⁵ Wackernagel, K. Z. 23. 467, Osth.

Perf. p. 175. Bezzenger's connexion (*Beitr.* 4. 314 f.) with Bulg. *setu*, Skt. *sāman* is impossible. Möller, K. Z. 24. 474 n. seeks to justify the loss of the guttural.

where the older editions alter $\epsilon\iota$ into $\omega\varsigma$. The sentiment 'if indeed it is true' is clearly inappropriate, or weak at the best, but, giving the word its original sense, 'if I may say so' is a perfectly natural softening of the foregoing vaunt, especially in the case of a man praising his own father.

$\epsilon\tau\eta\varsigma$ is another derivative of this root, but its history is not clear because it is uncertain which of its two main senses 'comrade' and 'tribesman' is the older. Probably they come independently from the root-meaning, the one from the daily intercourse of speech, the other from the right of speaking in the assembly¹.

It is difficult not to believe that this root was originally identical with I. Eu. $\chi\epsilon$ 'breathe' 'blow', Skt. $vā-$, Gk. $\alpha\eta\mu\iota$, but their different treatment in Gk. points to an early separation.

The evidence I have to offer for the **F** is therefore as follows: First, the frequency of hiatus before it in Homer. Secondly, Lat. *vērū*s. Thirdly, $\epsilon\tau\eta\varsigma$, which undoubtedly has the digamma (cf. I.G.A. 110. 8). Fourthly, the passages in Homer which show elision before it can scarcely be held to disprove the **F**, especially as in all but one (Υ. 255) the elision is of a particle. The new sense given to $\epsilon\tau\epsilon\acute{o}\nu$ makes the omission of the particle in some cases more easy, e.g. H. 359 (M. 233),

$\epsilon\iota \epsilon\tau\epsilon\acute{o}\nu \delta\eta \tau\omicron\upsilon\tau\omicron\nu \alpha\pi\omicron \sigma\pi\omicron\upsilon\delta\eta\varsigma \acute{\alpha}\gamma\omicron\rho\epsilon\acute{\upsilon}\epsilon\iota\varsigma$
 $\epsilon\acute{\epsilon}\xi \acute{\alpha}\rho\alpha \delta\eta \tau\omicron\iota \epsilon\pi\epsilon\iota\tau\alpha \theta\epsilon\omicron\iota \phi\rho\acute{\epsilon}\nu\alpha\varsigma \omicron\lambda\epsilon\sigma\alpha\nu \alpha\upsilon\tau\omicron\iota,$

can be translated 'If indeed we are to say "you address him in earnest" then verily the gods themselves, &c.' Υ. 255 in which correction is impossible is in a spurious passage (251—5) marked as such by Aristonicus.

The comparison of $\acute{\alpha}\nu\epsilon\upsilon$ with Skt. *sanutar* is also untenable. The parent of the latter is *sana-* which seems to imply originally 'duration in time' (Gk. $\epsilon\acute{\nu}\omicron\varsigma$, Lat. *senex*, Eng. *syne*). The meaning 'old' comes naturally from this. The suffix *-tar* is identical with $-\tau\epsilon\rho$ in Greek in $\acute{\alpha}\tau\epsilon\rho$ $-\tau\epsilon\rho\omicron-$ &c., and appears in *prātar*, *antar* with the same function as in *sanutar*. Its force

¹ This word is not connected with $\epsilon\tau\alpha\acute{\iota}\rho\omicron\varsigma$ in spite of J. Schmidt, K. Z.

25. 143, for the latter shows no trace of *f* in Homer (Knös pp. 221—2).

seems to be that the root to which it is added is contrasted with something else, whence its comparative function, and thus *sanutar* comes to mean 'far-removed'—but it is clear that the root could not give the sense without such a suffix, and that the suffix *-neṽ* could have this force is entirely unsupported by evidence. On the other hand, the comparison of *ānev* with Gothic *inu*, M. H. G. *ān*, O. S. *āno* (see Kluge, *Etym. Wört. s. v. ohne*), is justified both by form and by meaning. *ānev* then would stand for *ṇneṽ* and so be connected with the negative particle.—*ἐνιοι* has been influenced by *ἐνι*, *ἐναλίγκιος* by apparent, or actual, composition with *ἐν*.—*εἴρειν* is from the root *ferp*- see p. 38: Knös p. 176 is compelled to postulate a root *svar* in order to bring this into connection with *sero*.—*ὀρός* is not to be compared with *serum*. It is connected with *ὀργή* and perhaps with Skt. *ar*- 'be in motion'; it therefore had no initial *s*.—The derivation of *Ἑρῖνύς* from *Saranyu*- is improbable. Mythologic names should always be identified with great caution, and here the phonetic difficulties are considerable, while it seems unlikely that *Ἑρῖνύς* is to be separated from *Ἑρις*.—*ἐρύεσθαι* is from *ferp*- not *σερ*-, see p. 38.—*ἄλσος* see p. 51.—*ἄλσο* &c. see p. 51.—*ἐάω* is a very difficult word which has not yet been cleared up, but the derivation suggested is not plausible enough to overcome the difficulty of the breathing.

Curtius (*Grundz.*⁵ p. 682 sqq.) gives substantially the same list of irregularities, the only additional instances being the perfects *ἔσταλκα ἔσπαρται*, &c. for which I need only refer to G. Meyer, *Gr. Gram.*³ § 544.

The comparison of *εὔ*- and *su*- I mention only because it seems to be accepted by Hübschm. (*Arm. Stud.* p. 37). *εὔ*- is of course the stem of *εύς* which is connected with Skt. *āyus*-¹. The only other exception which has any authority is *ἄτερ*=*sonder*². There does not seem to be any valid reason for preferring this derivation to an equally possible one from Skt. *antara*-, Germ. *ander*.

¹ Collitz, K. Z. 27. 183 sq.

² Brugmann, *Gr. Gram.*¹ § 200 (*Handb.*¹ II. p. 117).

b. Original *ī* becomes ' in Greek.

The following are adduced by Curtius, *Grundz.*⁵ p. 687, ὕμνες, ὄπτι, ὄφρα, and ἀγέεσσι τεμένεσσι Hesych. which he says belongs 'unzweifelhaft' to the root 'yag'. Of these the first two are Aeolic; the third loses its aspirate by dissimilation; for the fourth the root *ag-* 'drive' seems equally possible, but the instance is too dubious to carry much weight.

L. Meyer, p. 159, gives no further examples.

Christ (*Lautl.* p. 154) presents the following:—ἥμαρ with ἡμέρα, ἥιος=ἰήιος, ἥπιος=Skt. *yāpya*, ἥια, εἰαί=ζειαί.

These words are all uncertain and have been the subject of numerous conjectures. Ascoli¹ has connected ἡμέρα and ἥμαρ with Skt. *vas-*, *us-*, 'burn', and this is the most widely accepted view. The evidence for the digamma is however insufficient (*τήμερον*: *σήμερον* does not prove it). The derivation to which Christ alludes is presumably that which connects them with Skt. *yāman-*, and if Latin *Iānus* may be taken as proof that the original form of Skt. *yā-* was *īā* and not *īē* (Doric shows *ἀμέρα*, *ἄμαρ*²), this derivation is otherwise irreproachable. However I prefer to separate ἥμαρ from ἡμέρα, and refer the latter to √ *sam* seen in Skt. *samā* 'year', Zend. *hama* 'summer', O. H. G. *sumar*, the last of which may be identical also in suffix. The connexion in sense through 'the bright shining time' is easy.—The words ἥιος and ἰήιος are also of doubtful identity; ἰήιος may be derived from *īh* and, if so, ἥιος may come from the interjection ἦ, but it is unsafe in principle to go further and assume the identity of *īh* and ἦ thus admitting that *ī* may at one moment appear as *ī-*, and the next disappear, in one and the same word, especially when that happens to be an interjection.—ἥπιος is also a much-debated word. The earliest guess is Ebel's³, who makes it Latin *pīus* with prefixed ἦ-. Bechtel⁴ identified it with Lith. *opūs* (*āpus*) *opīa* 'fragile'. The derivation which Christ ac-

¹ K. Z. 17. 403 sqq. *Stud. Krit.* II. 397 sqq. It is the defence and not the derivation which is due to him, cf. K. Z. 12. 310.

² G. Meyer *Gr. Gram.*² § 44. The only other connexion which avoids this

objection is Möller's, K. Z. 24. 474, with Skt. *ahan-*, and it is impossible on other grounds.

³ K. Z. 4. 447.

⁴ Bezz. *Beitr.* I. 164.

cepts—from Skt. *yāpya*—was propounded by Pictet¹. It seems to have met with a good deal of acceptance, but in my opinion Aufrecht's² is much superior. He connects it with Vedic *āpya* relationship, *āpi* kinsman, and so with the root *ap-* 'connect'.—The concluding instance does not bear upon my subject, for if *ξειαί* were = *ειαί*, the *ζ* proves that it would be a case of lost 'j-' (not *ξ*), which is even more improbable.

c. Original *sy-* becomes ' in Greek.

The instances alleged are:—*αι*, *ει* = Osc. *svai svae*—*ἄσμενος* from *ἥδομαι*—*ἔθος*, Skt. *svadha*—*ἔτης* also with *sva*—*ἦδος* beside *ἦδύς*—*ἴδιος*, Skt. *sva*—*ἴδος* √ *syid-* *ιδρῶς*, &c.

Brugmann³, though with some hesitation, accepts the first of these instances. His attempt however, to explain the loss of the aspirate, carries very little conviction. A little attention to inscriptions would have proved the derivation impossible at once. On the Gortyn inscription for instance *αι* occurs frequently: so do numerous other words where initial *sy-* is undoubted, as *ἐκαστος* (I. 9) *ὅν*, *ἐκάτερος* (I. 17) &c.: in all of these the digamma is consistently written; from *αι* the digamma is as consistently absent. The conclusion is obvious.

That *αι*, *ει*, come from some pronominal stem is highly probable: whether it is the same which appears in Skt. *idam id itas*⁴ in *e-* (*esas*) &c. in Latin *is*, *id* and in Greek *-ί* must remain an open question.

ἄσμενος is assigned to *ἥδομαι* as perfect participle by the Etym. Mag.⁵ which is followed by all authorities down to G. Meyer *Gram.*² § 530, who regards it however as sigmatic aorist. This derivation is impossible both from the absence of the aspirate and from the *a* which ought to be *η*. I believe *ἄσμενος* to stand for *Ἔν-σ-μενος* and so connect it with Skt. *van-* 'desire', Lat. *Venus* &c. As regards the *F*, the word occurs three times in

¹ K. Z. 5. 42.

² *Ib.* 5. 359 sqq. cf. Goebel, *Ib.* 10. 399.

³ *Gr. Gram.*¹ § 201. 1. [In Ed. 2 p. 225 he accepts Mr Darbishire's correction. C.]

⁴ Whitney, § 502.

⁵ Lentz, *Herodian* II. 358. E. M. 155. 23 *ἄσμενος*· ἦδω τὸ εὐφραίνω, ὁ παθητικὸς παρακείμενος ἦσμαι, ἡ μεταχθὲ ἡσμένος, καὶ ἄσμενος τροπῇ τοῦ *η* εἰς *α*.

the Odyssey¹, always at the beginning of the same line, and twice in the Iliad² with paragogic *ν* preceding (Dindorf: Bekker wrote *ῥάσμενος*). The termination *-σμενος* is most frequent with perfect participles, but there is no doubt that it was 'abstracted' as a suffix, and not formed independently in each case from the 3^d sg. (Brgm. M. U. i. 81 *n.*). It is also possible that Meyer may be right in regarding the *σ* as the sign of the sigmatic aorist. The accent follows that of other—not perfect—participles.

In *ἔθος* the aspirate is lost by dissimilation.—The derivation of *ἔτης* from *σφε-της* 'one's own man' and so 'companion' is most improbable. The suffix *-της* has a perfectly explicit and well-defined function; it forms nouns from verbs, and to have it added to a pronominal stem in this way would indeed be remarkable. Hence the derivation from *ῥημί*, suggested above, is to be preferred. The same objection applies to *ἴδιος*, which according to the received view is all termination, with the pronominal stem prefixed only to vanish. It is better to derive it from the root *uid* 'know' which gives the sense satisfactorily. The *F* is abundantly proved by inscriptions³.—*ἦδος* and *ἴδος* may be taken together. The latter is treated of by Osthoff⁴ who explains the loss of aspiration as due to dissimilation in the oblique cases, as *ἰδεῖος*. The same would then apply to *ἦδος*. This is possible, but on the other hand *ἔλος* under exactly the same conditions does not lose its aspirate, and so it is more probable that we have here a case of sentence doublets⁵ like those given by Brugmann *Grundriss* i. § 589. 3 (p. 447)—*uid*, *uād* beside *suid*, *suād* as *teg*, *pek*, *mer*, beside *steg*, *spek*, *smer*.

§ 4. Before passing to the second class of exceptions, it is convenient to bring together those cases in which the presence or absence of the aspiration is not persistent. In some of these both forms are well attested, in others we have only the

¹ i. 63, 566. κ. 134.

² ε. 108. τ. 350.

³ e.g. *Tab. Heracl.* i. 14. *ἐνδοθ'*
ἰδῖαν which Fabretti read on the
Gortyn inser. ii. 11 is corrected *ἐνδο-*

θῖδῖαν (Bücheler, *Rh. Mus.* Vol. 40
Suppl. pp. 13.—14, Fick, *Ilias* p. 559).

⁴ *Perfect*, p. 479.

⁵ [But see Excursus III. p. 58
inf. C.]

doubtful testimony of late grammarians: the latter are distinguished by an asterisk.

ἄγος	ἄγος	ἄμμος	*ἄμμος ¹	ἐμός	ἐμός ⁴
ἄδην	ἄδην	ἀνύω	ἀνύτω	ἐνος	ἐνος
ἀθρέω	ἀθρέω	ἄρκυς	ἄρκυς	ἔρση	ἔρση
ἀθρόος	ἀθρόος	αὖω	αὖω	ἔρσις	ἔρσις
ἀλέα	ἀλέα	ἀψίς ²	ἀψίς	*ἐσμός ⁵	ἐσμός
ἀλής	ἀλής	ἐδώλιος	*ἐδώλιος ³	ἔστε	*ἔστε ⁶
ἀλύω	ἀλύω	εἰλικρινής	εἰλικρινής	εὖω	εὖω
cf. ἄλη		ἐλύω	ἐλύω ὅλοι-	ἦθμός	ἦθμός
ἄμαξα	ἄμαξα		τροχος ἔλιξ &c.	ἦθω	ἦθω
ἄμη	*ἄμη	εἶργω	εἶργω	ἴστωρ	ἴστωρ
ἀμῆ	ἀμῆ	ἐλλός	ἐλλός	ὄρπηξ	ὄρπηξ

There are three causes to which this want of uniformity may be assigned.

1. A tendency to misplace the aspirate which is observable in other languages also, and in face of CIA. I. 324, cannot be denied for Greek⁷. In particular, as the distinction [in sound] between aspirated and unaspirated vowels was lost, misplacement [especially in writing] would naturally become more easy, and hence a breathing which *only* rests on late tradition is not to be held of much value. Generally speaking however the principle is of unsafe application, and should only be a last resort. Of the above, the only word which cannot be otherwise explained (neglecting those of doubtful authenticity) is ἄμαξα, ἄμαξα of which the derivation is not fixed.

If from ἀμ- and ἄγω the aspiration is correct and might be lost through the compound ἀρμάμαξα.

¹ So Liddell and Scott: Passow does not mention it. It seems to rest on ὄφραμμος in Theophrastus.

² 'Ionic' Passow.

³ Only Schol. in Ar. Av. 884.

⁴ *Lex de Spir.* 217 gives a popular derivation from ἐ, μέω which explains the origin of the aspiration.

⁵ Only a false MS reading according to Passow.

⁶ So Etym. Magn. The MSS of Theocr. waver. Probably aspirated on analogy of ὥστε or as=ἔως τε. That ἐς τε is the true derivation is proved by ἐν τε on the Delphian inser. Caer² 204. 40.

⁷ [Cf. now Meisterhans, *Gramm. Att. Inschr.*² § 38, 32 with Brugm. *Gr. Gr.*² p. 73. C.]

2. The word has one breathing by rule, but beside it and connected with it in sense or system stand words which, also regularly, have the other breathing. Then assimilation takes place in one direction or in both. Thus *ἀθρόος* loses its aspiration by the rule of dissimilation, and regains it by the influence of the other compounds with *ἀ-*.

This word seems to have been followed by *ἀθρέω*, although it is not easy to trace the semasiological connexion. The unaspirated form is of course regular, whatever the derivation. Kluge's¹ ingenious identification with Teut. *wun-dro-* is impossible as the Homeric evidence is conclusive against the F (see K. 11, M. 391, Ξ. 334, μ. 232, τ. 478).

So *ἀμῆ* is influenced by the loss in *ἀμόθεν* as explained above (p. 26).—So *ἦθω* loses its aspiration by the same law and regains it from a popular connexion with *σῆθω* which in itself is doubtful [see p. 60 footnote 1]. It is better to connect it with *αἴνω*, thus *ἦθω* = *sn̄-θω*², *αἴνω* = *sn̄-i-ō*.

The aspirated form was predominant in the time of the scholiast on Ap. Rhod. i. 1294, who therefore prescribes *ἦθμος*. For this form the evidence of the Sigeian inscr. (I. G. A. 492, Roberts, *Introduction*, 42) is adduced. The upper (Ionic) inscription has *ἦθμον*: the lower (Attic), *ῆθμον*, unless the H was a slip in copying from the one above, and E added as a correction: compare I. G. A. 446 and 482.

This explanation also holds for the variation in *αῦω* and *εὔω* as is fully set forth by Osthoff³. The unaspirated form *ἔρση* is due to the epic *ἐέρση* in which the *ἐ* is anaptyctic and so unaspirated.

Slightly different are the following cases, in which the influence is exerted by words of similar meaning. *ἄδην* I derive like *ἄσμενος* from *uen-Fn̄-δην*—and trace the aspiration to *ἄλις*, or *ἀνδάνω*. For the digamma compare

ἀλλ' ἔτι μὲν μὴν φημι ἄδην ἐλάαν κακότητος.—ε. 290⁴.

¹ *Etym. Wörterb.* s. v. *Wunder* "das germ. *wundro-* scheint mit gr. *ἀθρέω* (für **φαθρέω*) 'sehe, schaue, betrachte', auf ein idg. Wz. *wendh* 'anschauen, anstaunen', zu weisen."

² For the formative suffix cf. *πλή-θω*, *μυό-θω*, &c. *Brgm. Gr. Gram.*¹ § 116. V. Henry, *Comp. Gram.* § 92.

³ *Perf.* 478—493.

⁴ Against the digamma is T. 423.

So ὄρπηξ is the true form. It is to be connected with ὀρ- 'swell' as in ὀργή and ὀρός (*supr.* p. 29). The aspiration is due to ἄρπη¹ as the young shoots had to be pruned.

3. The confusion is due to the intermixture of different roots. That is to say: two words of different origin and with different breathings, but otherwise identical in form, bear similar meanings; the result is a complete fusion of the two and the aspiration varies.

An example in which the distinction is observed is given by Tzetzes (p. 946, Müll.) Ἐπωπίδα. Δασέως μὲν ἐπωπίς, ἡ ἀκόλουθος καὶ δμῶς ψιλῶς δὲ ἐπωπίς, ἡ ἐπισκοποῦσα καὶ βλέπουσα παρὰ τοὺς ὦπας. A well known case of complete fusion, is ἔχω from *ueġh* and *seġh*².

This principle accounts for a good many of the above doublets. ἄγος and ἄγος are referred to different roots by Curtius³. Much confusion is caused by the roots *sel* and *uel*. Of these the former opens up a difficulty. The rule is generally laid down that Gk. ρ, λ, and Armenian *r*, *l*, correspond to each other and to I.Eu. *r*, *l*. This needs modification⁴. The full system of liquids possessed by Armenian seems to prove that the original language had also two 'r's (and two 'l's), one of which would become in Greek ρ or λ, the other only ρ. This renders possible identifications which have great inherent probability, as Armenian *arag* with ἐλαφρός, Arm. *arew* with ἀλέα, and *ser* with *sel*. Compare also Cretan ἀφαιλήσεσθαι with the ordinary αἰρέω.

ὀρμή &c. point to the root being *ser*, Skt. *sar*; but the almost certain derivative of this, *saras*, compared with ἔλος⁵ and *salila*, points to *sel*.

¹ More probably than ἔρπω.

² An interesting subject for investigation would be, how far these two roots can be separated in Homer by means of the digamma. For instance in A. 51 βέλως ἐχεπευκέτς, the *f* is present and the sense also points to *ueġh*: on the other hand, in line 113, ἄκοι ἐχειν must be scanned ἄκοι ἐχειν (Hartel *H. St.* 3. 41), the digamma must be absent, and the sense is that of *seġh*.

³ *Grundz.*⁵ pp. 170-1.

⁴ [Mr Darbishire's view on this

question seems to have been modified; see *The Sanskrit Liquids*, pp. 210, 236, 244 and 256 *inf.* C.]

⁵ Compare also its derivative ἐλκη, with Lat. *salix*, Teut. *salahā*, English (dial.) *sally* 'the willow', which is therefore 'the water-tree'. Exactly parallel is the connexion of Lat. *alnus*, Eng. *alder* with Skt. *ar*. 'move'. The common equation of ἔλος with *Velia* is impossible from the absence of *f* in Homer, see Knös p. 80.

The idea 'move' can be distinctly traced in *ἐλιξ ἐλίσσω* *ἐλμινς* and *ἐλύω*, as well as in *ἔλος* just mentioned. It is also visible in the 'secondary' root *selk* whence *ἔλκω ἔλκος ὀλκή* Latin *sulcus*. *ἔλκω* is thus to be separated from Lith. *velkù* Lat. *ulcus*, although Greek may have fused the two roots; but the unaspirated form has left no trace, nor does Homer show the digamma¹, see however *infr.* p. 49.

The derivatives of the root *uel* implying 'motion round', Latin *volvo*, have no aspiration, e.g. *εἴλω, ἄλη ἀλάομαι ἄλαος*, &c. Hence the confusion in *ἐλύω ἐλύνω, ἀλύω ἀλύω, εἰλικρινής εἰλικρινής* ('tested by shaking'), *ὀλοίτροχος ὀλοίτροχος*.

The explanation of the doublets *ἐλλός ἐλλός* is similar. There are two words of this form, and in both the aspiration is unstable. *ἐλλός* 'a young deer' is the true form as proved by the connexion with *ἐλαφος* Arm. *ελη*². The adjective *ελλός* is generally supposed to mean 'dumb', which appears to rest on the fact that fish are called *ελλοί* by Sophocles *Ai.* 1297 and *ἄνανδοι* by Aeschylus *Pers.* 579. The other meaning given by Hesychius, 'swift', is even more appropriate as an epithet of fish³ and gives an intelligible derivation which the other does not. Assigning it then, with this sense, to *sel* move, the aspiration is regular. The two words *ἐλλός* and *έλλός* being otherwise identical in form then interchanged breathings, so that the tradition varies in the case of both. The meaning 'quick, lively', for the adjective explains this better than 'dumb'; for the active motions of a young animal are much more characteristic than voicelessness.

έλλοψ does not make against this sense. The second member of the compound is passive as in *στέρ-οψ* beside *ἀστήρ stella*. For the breathing cf. Hesych. "*έλλιπόντες τῆς ὀπός· καὶ δασείς*" where *καὶ δασείως* should be read.

Quite distinct from the root *uel* just mentioned is another root which appears in Greek as *Feλ* and means 'press, compact'. It is distinguished from the other by forming one of that class

¹ Knös p. 79.

² Brugmann, M. U. II. 173; also Windisch K. Z. 27. 168 who compares Skt. *ēṇa*, Lith. *ėlnis*, Cymr. *elain*.

³ Cf. *τερὸν λχθόν* II. 407, where *τερὸν* = *iṣiram* from *iṣ* 'move quickly' Grassm. 221; also the German phrase 'gesund wie ein Fisch'.

of words in which F becomes ' under conditions hitherto unexplained.

From this root come *άλις αλία αλίζω*¹, &c. and probably *έλωρ έλειν αλώναι*, &c.

Possibly also in *όχλος* which may be *Φολχλος* (for the loss of λ cf. *Grds.* § 266) and so to be compared with Skt. *vlāg.* = *vlg.*, but the reason for the χ is not apparent².

From this root *άλής* must be derived, and hence the spiritus asper is correct. The form *άλής* is due to supposed contraction from *αολλής*, for which reason the α was wrongly lengthened (Call. *Fr.* 86). This lengthening separated the word from *άλις* which therefore shows no variation.

αολλής is another derivative of the same root and is not to be connected with Lat. *sollus* (= Skt. *sarvas* = *σλος*), but as Roehl³ surmised, to be compared with *άΦλανέος* in Elean Cauer² 259. 4, I. G. A. 113. c. The two words differ only in sentence-accent, *σπ-ν]-νεϋ-* becoming *άΦλανεF-*, *σπ-ν]-νεϋ-* *άΦολνεF-* which passes into *ά-όλλ-ης*, losing its initial aspiration by dissimilation, as *αϋ* and *αοσσητήρ*⁴. Of course the inscription shows *άΦλανής* being Elean.

There is yet another root in Greek of the form *Feλ*, and it also shows the aspirate in its derivatives. The meaning is 'shine, burn' and it appears to be identical with Armenian *vař* of the same meaning. From it come *έλη έλάνη* (? *Έλένη*) *είλη* &c. It is by analogy with these words that *άλέα* (most probably *άλεFa* Arm. *arew*) receives its aspiration.

This root also explains *ήέλιος* rationally, for of course the ε cannot be 'developed' by the η. *ήέλιος* is for *ή-Feλ-ιος*, where the η- is of the nature of a preposition or verbal prefix as in *ή-δυνάμην* &c. (Osth. *Perf.* p. 129 n.) and so the whole word is analogous to *ω-κεανός*⁵. In *άέλιος*⁶ (cf. *άβέλιον* &c. G. Meyer, *Gr. Gram.*² § 235

¹ The form is given by *καταέλεμένων* on the Gortyn inscr. (Cauer² 113. 13), compare *έγφηληθίωντι* Tab. Her. i. 152, *άποφέλοι*, Cauer² 255. 7.

² [But see further p. 231 inf. C.]

³ I. G. A. p. 178.

⁴ Curtius *Grundz.*⁵ No. 460, Brugm. *Grds.* i. § 135.

⁵ It is no proof that Greek η- cannot

be Skt. *ā-* to allege that Greek ω- is Skt. *ā-*: ω- and η- are a case of vowel-gradation (Ablaut) and become alike *ā-* in Sanskrit.

⁶ The root *sāmel-* assumed by J. Schmidt, K. Z. 26. 9, is quite arbitrary for Greek and presents serious difficulties.

which proves the *ʃ* the *a* is prothetic (anaptyctic) and so *short*, see the metrical evidence in Liddell and Scott. *ἄέλιος* is a mistake from *ἡέλιος*. The smooth breathing in *ἀέλιος*, *ἡέλιος*, and *ἀλέα* seems to have produced *εἴλη* beside *εἴλη*.

The variation between *ἀνύω* and *ἀνύω* is explained by Osthoff¹ as due to dissimilation from the 2nd sg. pres. indic. mid. of the root *ἀν-*,—*ἄνεσαι* becoming *ἄνεhai*. This is very improbable, for that form must have been of rare occurrence. The verb is cognate with Skt. *san* meaning 'accomplish, obtain': but beside this stands another root which approximates to the same sense, namely *van*, meaning 'desire, obtain', the existence of which in Greek has not hitherto been pointed out. As a matter of fact it has a large number of cognates; besides *ἄσμενος* and *ἄδην* which have already been mentioned, it forms *ἄμεναι*, *ἄσω*, *ἄσάμην* and the other forms generally referred to an imaginary present *ἄω* 'take one's fill'; so also *ἄται*² and *ἄτος* with anaptyctic *a*. This root then gives *ἀνύω* as *san* gives *ἀνύω*.

Similar is the case of *εἶργω* and *εἶργω*. The former is connected with Skt. *ṛj* the latter with *vrj*. The distinction in sense between these two roots, mentioned by Grassm. 1326, finds its reflex, although considerably obscured, in the canon laid down for the use of the aspiration in Greek. *εἶργω* through some derivative with *κ* (e.g. *εἶρκτή*) produces *ἔρκος* and *ἄρκυς*.

ἔνος is connected with Skt. *sana-* and *ἔνος* with Skt. *anya-* Lat. *per-en-die* by common acceptation; but *ἐν* in *ἐνὶ καὶ νέα* is generally identified with the former sense.

ἔρσις and *ἔρσις* result from the two roots *φερ* and *σερ*, the former giving *εἶρειν* *εἰρήνη*³ &c. the latter *ἔρμα* &c. as mentioned above (pp. 22, 29).

§ 5. It appears then, that dissimilation and analogy account for all the certain cases in which *s-*, *z-* disappear without leaving the spiritus asper, and that in the group *sz-* a third explanation is possible, resting on the hypothesis of *sz-* *z-* doublets in the original language. All derivations, which cannot be ex-

¹ *Perfect*, p. 479.

³ G. Meyer, *Gr. Gram.*² §§ 65, 35.

² *Hes. Scut.* 101.

plained in one of these ways, have been shown to be wrong, or at least to admit of alternatives, which by rule have the probability on their side¹.

It remains to touch upon some words which show the spiritus asper although the received etymology does not bring them under any of the above five rules.

L. Meyer, p. 636, connects *αῖρέω* with *αἰτέω*, *αἴτιος*, *αἴνυμαι* and Latin *aeruscare*, which is absolutely improbable. A more accepted explanation which connects it with Skt. *vr̥-* 'choose' is disproved by the absence of F in Homer², nor is it to be connected with *αἶρω* (Skt. *ar-* 'set in motion')³. *αῖρέω* I regard as *īr̥-i-eiē-ō*, with its aspiration from *ī-* by Rule 2. The root *īer-* is seen in Skt. (bye-accent form) as *īr-* (Grassm. 233) and in meaning it is akin to *ar-*. The two roots need very careful distinction and for Greek the aspiration is the only test. Thus *ἄρμα* *ἄρμος* *ἄρμονία* are from *īer-* while from *ar-* come *αἶρω* and *ἄρμενα* on the Delphian inscr. Cauer² 216, which means 'moveable' and so 'null and void' as opposed to *μόνιμος*.

Probably not in *ἄραιός* as the evidence shows the F (Knös p. 174). For *ar-* in Latin cf. Brgm. *Grds.* I. § 499.

ἄπτω is not to be connected with Latin *aptus*. The derivation given by Christ⁴ from Skt. *saj* is quite satisfactory and Clemm, *Curt. Stud.* 9. 416, brings no evidence to invalidate it.

ἐννέα is to be explained as *en-neuṃ*⁵ where *en* is the preposition. This is better than *enevn*⁶ and than supposing connexion with *ἐννῆ* = 'nine'⁷; but no one of the three derivations explains the aspiration. On the Tab. Heracl. the form is at once explicable by the occurrence of *ὀκτώ* which proves that *ἐξ ἐπτά* have produced *ὀκτώ ἐννέα*. But *ἐξ ἐπτά* could not produce *ἐννέα* directly. Therefore as *ἐννέα* occurs in Attic it is necessary to assume that *ὀκτώ* also did occur, on the evidence of *ὀγδόη* CIA. I. 325, 13.

¹ Hence derivations like 'Ἀπόλλων the F.
= *Saparyeṇya* (K. Z. 29. 193 ff.), *ἀμνίον*
= *sanguen* (ib. 257) are unwarranted.

² Knös, p. 172.

³ Brugmann (K. Z. 27. 196) offers another explanation, which requires

⁴ *Griech. Lautl.* p. 132.

⁵ Wackernagel, K. Z. 28. 132.

⁶ G. Meyer, *Gr. Gram.*² § 405.

⁷ Wackernagel, K. Z. 25. 263.

ἐορτή = Skt. *vrata*¹. Here the ε- is anaptyctic² and so should be unaspirated: *vr̥ta*- however passed directly into ὀρτή, which was falsely supposed a contraction and so produced ἐορτή.

The proper orthography for Homer is therefore ἐορτή, and so ἐορτή : ὀρτή exactly = ἐέρση : ἔρση.

The notion that ἡγέομαι is the causal of ἄγω is a mistake. It is the causal of a root *iaġ* which appears in Skt. (bye-accent) as *īj*, and as *yaj*. The original sense of 'set in motion, move' is retained by *īj* and ἡγέομαι, but *yaj* is specialised for the sense of 'move by sacrificing' (Grassm. 1070 No. 4).

īj was then brought into relation with *aj*, just as *īr* mentioned above (p. 39) was referred to *ar*, and these two then seem to have formed a type whence *īç* from *aç* &c.

ἦκα = *āça* Osthoff, P. and B. B. 8. 290. *n.*, and Perf. p. 170, is another derivation which will not bear scrutiny. The assumed root is *aġ* 'penetrate', but this is impossible, for the forms in Skt. show that *aç* stands for I. Eu. *ġġ*, and therefore the reduplicated stem *āç* must be comparatively late. As even Brugmann³ and apparently G. Meyer⁴ accept this explanation, it must be supposed that, against the probabilities, they separate *āça* entirely from *aç*, and presumably explain the aspiration by the influence of *īημυ*. On the other hand the view which makes ἦκα the perfect—with or without reduplication—of *√sē*⁵ does not appear open to any serious objection.

Of course, in either case, the relation of ἦκω to ἦκα is the same.

ἦμαι = Skt. *āse*, is at once explained by the parallel root *sed*⁶.

ἱερός = Skt. *iṣīras*. First propounded by A. Kuhn⁷, whose theory to account for the aspiration is an ingenious one, which

¹ Fick, *Wörterb.*³ i. 211.

² G. Meyer, *Gr. Gram.*² § 101.

³ *Gr. Gram.* § 134.

⁴ *Gr. Gram.*² § 562, but contrast

§ 527.

⁵ *Mém. Soc. Ling.* v. 136, cf. Osth.

Perf. p. 382.

⁶ Osth. *Perf.* p. 108.

⁷ K. Z. 2. 260 sqq.

he endeavours to support by numerous instances, namely, that *s* became *h* and then passed over the vowel. His examples in which the *s* is followed by a consonant, as *ἡμεῖς*, *ἡμῖν*, *ἡμῶν* are erroneous, as the theory that *s* became *h* in this position has long been abandoned. Where the *s* stands between vowels, the theory is more attractive, as it gives a convenient explanation of *ἡμῖν*, which by the rule for dissimilation ought to become *ἡμῖν* from *ἡμῖν*. But even here, it cannot be accepted, for three reasons:—(1) There is no phonetic reason for the transference, (2) All the examples may be explained otherwise, (3) It ought to hold, and does not, for *ἰός* beside Skt. *iṣu-*, *ἰοσσητήρ* from I. Eu. *sm-soq-*, and others.

His only really strong instance is *ἰερός*. The equation Boeot. *ἰων* = *ἰων* = Skt. *aham* is impossible: *ἰων* is probably after *ἡμεῖς* *ἰμεῖς* and the 3 sing. *ἴ*. In *ἡμῖν*, form-association restores the aspiration.

To explain the rough breathing in *ἰερός*, we must take into account the collateral form *ἰρός*, which, it is needless to remark, cannot be the result of contraction¹. Osthoff's attempted explanation², with its necessary infringement of the law which he and Sievers formulated, is for that very reason open to grave suspicion. Both words are found in Homer without distinction in meaning, but they are really separate in origin. Corresponding to *iṣiras* the true form is *ἰερός* (*ἰαρός*) beside which stood a derivative of the root *vī*—*ἰρός*, with meaning practically identical: even in Sanskrit the difference in sense between *iṣ* and *vī* is very slight. As was inevitable, the two words were assimilated—*ἰρός* seems to have its accent from *ἰερός*, and the latter received from *ἰρός* the quantity of the *ι*³ and the rough breathing.

If this hypothesis is tenable it explains the striking uncertainty of spelling in this word in Cauer² 204—*ἰεπο*-*ἰαπο*- and *ἰαπο*- side by

¹ Curtius K. Z. 3. 154 compares *πδλεῖ*—*πδλι* which even if it were true—cf. Brgm. Gr. Gram.¹ § 82— would not be parallel.

² Perf. pp. 439—40.

³ Not consistently, but e.g. in P. 455.

side. The two words are kept distinct on the Cyprian inscriptions—*i·ro·ni*¹ Coll. 60. 8. 31 but *i·e·re·o·se* ib. 38. 3.

ἴμερος is also to be derived from *vī*, and not from *is*, which has its proper representation in *Ἰσμήνη*.

The common parallel *ἰκνέομαι* : *οἶκος* : Skt. *viç-* has often been disputed, but the alternative suggested by L. Meyer², who refers it to Skt. *aç-nō-mi*, is an unhappy one, for, as was pointed out above, *aç* stands for *ḡk* so that the passage from *ακ-* to *ικ-* in Greek which Curtius³ justly calls 'sehr singular' becomes impossible. Equally untenable is Bréal's theory⁴ of its derivation from *yā* 'go' with suffix *-k* as in *ὀλέκω facio* &c., while the ordinary derivation is met by the fatal objection that Homer shows no trace of the *F*⁵. It seems preferable therefore to trace *ἴκω* to the root shown by Osthoff⁶ to exist in *ἵκταρ*, *ἐνίπτω*, *ἐνιπή* and Latin *icere*. In this case, the *κ* in *ἰκνέομαι* must have been restored, for the velar ought to become *π* before the nasal. The aspiration is to be accounted for by the close parallelism of *ἵκω*, and has extended to *ἵκταρ*.

To render this explanation probable, isolated forms of the root ought to show the smooth breathing, and instances are not wanting. The root-meaning as shown by Osthoff, is 'strike', from which 'reach, arrive' are developed in *ἰκνέομαι*, but in *ἵκμενος*⁷ the slightly different one of 'suing' (compare *vor-treff-lich*, &c. in German). In this sense therefore *ἵκμενος* remained unaspirated, and the same is true of the Hesychian glosses *ικτᾶ*⁸ and *ἵκαρ*⁹. There is nothing to show whether *ικανός* belongs to this root or to Skt. *vyac-* 'umfassen' (Grassm. 1357) and so 'capax', 'able'. In either case the aspiration must be unoriginal, if any stress is to be laid on *ἵκτεν κρατεῖς, Λάκωνες* in Hesychius¹⁰.

The ordinary form *ἵστωρ* is not accounted for by its almost certain derivation from *Fiδ*. The Homeric form is *ἵστωρ*

¹ Cyprian had no sign for *h*.

² K. Z. 23. 49.

³ Stud. 6. 414.

⁴ Mém. Soc. Ling. v. 155.

⁵ Knös p. 123.

⁶ K. Z. 23. 85.

⁷ e.g. A. 479: compare *ἵκμενον· εἰ μὲν*

δασέως, τὸν ἵκτιον λέγει οἶον πορευτικόν...

εἰ δὲ ψιλῶς, τὸν ἵκμαδῶδη καὶ ἐνικμον οἶον

ἐνυγρον Hesych. M. Schmidt Vol. II.

p. 353.

⁸ l. c.

⁹ l. c. p. 352.

¹⁰ l. c. p. 354.

(Σ. 501). Curtius¹ mentions a possible source of the aspiration in ἵστημι, with which it might be connected in the sense of 'umpire' 'arbitrator', and that the tradition is by no means unvarying as regards the aspiration. (Lentz, *Philologus*, Suppl. Bd. I. p. 700.)

ὀράω and ὄρονται. If these are to be connected it must be by the supposition of a doublet like those referred to on pp. 32, 48, 49; ὀράω may at least plausibly² be connected with Skt. *svar-* 'leuchten', while ὄρονται is commonly referred to O. H. G. *wara* Gk. ὄρα³, but an identification of these two roots is impossible. That ὀράω does not show F in Homer⁴ may be explained, with this etymology, on the lines laid down *infra*, p. 49.

§ 6. The above are a few instances of isolated derivations which rest on the assumption of inconsistency in the use of the spiritus asper, but more important than these—because to a certain extent justifying such an assumption—are those words in which the aspiration appears to represent older F, contrary to the usual rule that F becomes the spiritus lenis. In the foregoing pages, derivations involving each of these alternative equivalents have been accepted without comment, and therefore the discrepancy must be discussed, before any approach to an exhaustive treatment has been made.

A few of the best known instances for ' = f are:—ἐάνός Skt. *vasana*, ἔκητι, ἐκών Skt. *vaç-*, ἔννυμι Lat. *vestio*, ἔρη Skt. *vrç*, ἔσπερος Lat. *vesper*, ἑστία Skt. *vas*, ἦλος Lat. *vallus*⁵, ἱεσθαί (ἱρός) Skt. *vī-*.

The total loss of f scarcely requires illustration: ἄστυ, ἔαρ, εἴκοσι, ἔπος, ἔτος, ἰδεῖν, οἶκος, are a few of the commonest examples.

The examples for each alternative are too numerous for every case of either to be explained by independent analogy, and there is no particle of evidence (except the phenomenon

¹ *Grundz.* p. 686.

² [Mr Darbishire has put a query to this statement in the margin of his own copy. C.]

³ Hübschm. *Idg. Vocalsyst.* § 261,

p. 171.

⁴ Knös p. 142. Its absence in ὄρονται, &c. is explained by L. Meyer, K. Z. 23. 53, as due to the initial o- (?).

⁵ Wackernagel, K. Z. 25. 261.

itself) to support the view that every F passed through the spiritus asper to the spiritus lenis, for the examples do not differ in date.

One only possibility remains: that F had two phonetic values. The same then must be true for Sanskrit *v* and Latin *v*, and so these languages can give little or no assistance in making a distinction. The language for this purpose is Armenian.

In the examples which follow, Hübschmann's system of transcription¹ is used, except in one point which is more of consistency than of importance. His contention that a single sign in an alphabet ought to be transliterated by a single sign, may receive this addition, that two signs ought not to be transliterated by one, and therefore it is a defect in his system that he transcribes *աւ* by 'av' but *ալ* by 'u', and while 'v' is *վ* in *կով* 'kov' it is *լ* in *հաւ* 'nav'. It is better to use *w* consistently for *լ*, and keep *v* for *վ* so that *աւ* becomes *aw*, and *ալ* *ow*, i.e. *u*.

It is well known² that where Sanskrit and Latin show *v*, Armenian sometimes has *g* and sometimes *v*, *w*. The reason for this variation has not yet been discovered, and will not be, until more has been done for Armenian etymology on comparative principles, but the following words, in which the sound is initial, have fairly certain Greek equivalents.

1. With *g*.

Armenian	Greek	Skt. Lat. &c.
<i>gar garn</i> (64) ³	<i>Γαρν- ἀρν-</i>	<i>urāṇa-</i> Skt.
<i>gelmn</i> (67)	<i>Γελιον ἔριον</i> ⁴	<i>varman-</i> Skt.
<i>gin</i> (69)	<i>Γωνο- ὠνο-</i>	<i>vēnum</i> Lat.
<i>gini</i> (70)	<i>Γοινο- οἶνο-</i>	<i>vinum</i> Lat.
<i>gitem</i> (72) and thence <i>gtanem</i> (77)	<i>Γιδεω ἰδεῖν</i>	<i>vid-</i> Skt. and thence <i>vindāmi</i>
<i>go-m</i> (73) ⁵	<i>Γάστερ- ἄστν</i>	<i>vas-</i> 'dwell' Skt.
<i>gorc</i> (75)	<i>Γεργο- ἔργο-</i>	<i>werk</i> Teut.
<i>k'san</i> ⁶	<i>Γικατι εἰκοσι</i>	<i>viginti</i> Lat.

¹ *Umschr.* pp. 31—39.

² Hübschm. *Arm. Stud.* pp. 65 and 74, cf. *Brgm. Grds.* § 162.

³ The figures in brackets refer to the numbers in Hübschm. *Arm. Stud.*

⁴ *ἔριον* seems to have *f*, δ. 124, but *εἶπος* has not; δ. 135, i. 426.

⁵ Compare also ZDMG. 36. 119.

⁶ *K'san* for **g-san*, **gisan*, Hübschm. l. c. p. 65 note.

2. With *v*.

Armenian	Greek	Skt. Lat. &c.
<i>vaṣ</i> (262)	φελ-η, ελη (p. 37)	<i>virti</i> Lith.
<i>vasn</i> (263)	φεκ-όντ- ἐκόντ-	<i>vaç-mi</i> Skt.
<i>vël k'</i>	φηλο- ῆλο-	<i>vallus</i> Lat.
<i>vs</i> ('propter')	φεκ-ῆτι ἐκητι	<i>vaç-</i> Skt.

Here it will be noticed that those words in which the digamma passes into the spiritus lenis, have in Armenian *g* as the equivalent of Skt. and Lat. *v*; while those in which the digamma becomes the spiritus asper, have in Armenian retained *v*. The conclusion is that the same original distinction underlies both.

To postulate such a distinction for the original language, involves no innovation in the phonetic field. In the case of the corresponding (palatal) spirant such a distinction is made—it rests upon some slight indications in Sanskrit, but chiefly on the difference of representation in Greek when initial¹. On this analogy there has been already assumed a corresponding distinction—*v* and *u*—in the case of the labial spirant. If this is so, it is not surprising to find the distinction preserved under exactly analogous conditions.

The result of the foregoing examples may therefore be formulated as follows:—

	I. Eu.	Skt.	Arm.	Gk.	Lat.	Teut.	Balt. Slav.
1.	u	<i>v</i>	<i>g</i>	'	<i>v</i>	<i>w</i>	<i>v</i>
2.	v	<i>v</i>	<i>v</i>	'	<i>v</i>	<i>w</i>	<i>v</i>

[A most important modification of this view of the nature of the two I. Eu. sounds will be found below, p. 197. C.]

It might possibly be thought that Armenian *g* should represent original *v* (not *u*) in the same way that Greek *ζ* represented *j*, but this is a superficial objection and the above table may easily be shown to give the true correspondence. The distinction between I. Eu. *v* which cannot become *u* and I. Eu. *u* which can, has been much obscured even in Vedic, but the Skt. root *vac* shows the reduction to *u* in so many forms that the original form may be assumed as *ueq*: now this in Greek gives *επος*. Then in the

¹ Brgm. *Grds.* § 598.

root *va^zd* = 'water', the weak form *ud-* can be proved for the original language by Skt. *udan*, Gk. ὕδωρ. Therefore it should be written *ua^zd*: and this in Armenian gives *get* (68). The result is also what might be expected from the larger number of examples under Class 1, and agrees with another law, which seems to hold for Armenian, that I. Eu. *j* becomes *y* but *i* becomes *ǰ*, *j*—for the former compare *yoiz-k* 'agitation, ferment' with ζέω and Skt. *yas*-¹, for the latter, see Hübschm. *Arm. Stud.* §§ 19 and 22.

§ 7. The small number of examples under 2—the second and the fourth are practically identical—is no presumption against the truth of the inference which rests on (a) the high probability of those examples, (b) the absence of conflicting evidence. What there is of the latter can soon be examined, for as Armenian has not hitherto been the field for much rash theorizing, the number of unsafe conjectures is fortunately small. The theory could be upset by proving any one of the following four points:

- a. Armenian *v*- = Greek '.
- b. Armenian *v*- = Armenian *g*-.
- c. Greek ' = Armenian *g*-.
- d. Greek ' = Greek '.

These we will take in order.

a. No evidence adduced.

b. Also unsupported by facts. It will be observed that this only applies to the sounds as initial.

c. ἔσπερος = *gišer*² (71). This is by no means exempt from difficulty on other grounds. The rule that *š* can stand for *sq-* is invented to suit this solitary case and is a very unlikely one³. The chief argument is the similarity of meaning, which is equally well given by an alternative derivation. Armenian *giš-* can by the *regular* phonology of the language stand for I. Eu. *ghis*-⁴ which is found in Gothic *gis-tra-*, Latin

¹ For the *s* lost between vowels see Hübschm. *Arm. Stud.* § 44. 1). a.

² Accepted by Brgm. *Grds.* § 561.

³ Hübschm. l. c. § 34.

⁴ For *g* = *gh* see *Arm. Stud.* § 16, for

š = *s* compare *p'oši* = Skt. *pāmsu* (later *pāmçu*) *ib.* § 34. From Germ. *gestern*

Gk. *χθές* perhaps *ghes* should be the form. In that case for the *i* cf. *ib.* § 7.

Latin of course is indecisive.

heri (= *ghis-i*). For the suffix *-er* compare *j-er* from I. Eu. *i-u-*, *ardar*: Skt. *ṛta-*, *barjṛ*: Skt. *brhat-* (*Arm. Stud.* § 40).

The meaning presents no difficulty. In all languages with which I am acquainted 'morning' and 'to-morrow' are originally identical and it is natural to suppose the same relation between 'evening' and 'yesterday'. In Armenian itself we have *erek* 'evening', *evēk* 'yesterday'.

This is confirmed by Diefenbach, *Vergl. Wörterb. der gotischen Sprache*, Vol. II. p. 410 no. 34. "*gistra-dagis*...es bedeutet eigentlich vespera wie die meisten gestern bedeutende Wörter." Kluge s.v. *Gestern* (ed. 4 p. 113^b) remarks "Offenbar hatte das Grundwort die Doppelbedeutung 'morgen' und 'gestern' (eigtl. 'am andern Tage von heute aus gerechnet')," but the double meaning (cf. e.g. Ulfilas, *Matth.* iv. 40) is readily explained by adhering to the above sense 'evening' for the root, and tracing the 'otherness' in the comparative suffix *tra*.

ἐννυμι = *z-genum* (104). This is an extremely probable derivation and only an apparent violation of the rule. The distinction applies only to *g* and *v* when *initial* and as in this word the prefix *z-* is always present, the *v* is virtually medial.

The laws under which medial *v*, *w* and *g* interchange are as yet unknown but certainly do not depend on an original distinction. Thus *loganam* (124) and *lawanal*¹ alike = *lavo*; *arew* (41) has a derivative *aregakn*; *kov* (148) gives *kogi* (146): *čow* but aor. *cogay*². On the other hand a possible derivative of the root, from which the prefix is absent, is *vas* ('tela subtilis' Rivola) which thus follows the rule.

d. ἔρση : ἄρσην. The derivation of the latter from Skt. *vr̥ṣan-* (Bopp, Benfey) has now been given up. Curtius preferred *ṛṣa-* on the ground that no trace of *F* exists in Homer, which Knös (p. 184) will not admit to be decisive. The words are separated by Wackernagel (*K. Z.* 29, 127, 129), and compare Hübschm. *Arm. Stud.* (28).

No confusion of breathings on the lines indicated above (pp. 35, sqq.) arose between these representatives of *vr̥ṣ* and *ṛṣ*, because in Greek they diverged in vocalism and meaning³.

This is absolutely all the evidence against the rule.

¹ Rivola.

² ZDMG. 36. 118.

³ Ionic ἔρσην might be considered assimilation to ἔρση, but the ρ could

itself produce the change. Or it may be the strong form (G. Meyer, *Gr. Gram.*² § 314).

§ 8. A few new derivations which illustrate the rule may now be added.

Greek *αἶψα-ασιά*, 'wall of dry stones', Armenian *vēm*, 'petra, lapis'. Not quite certain, as no example is given of *ai* becoming Arm. *ē*, though *eḡ* and *oḡ* do. The variation may be in Greek: for the termination compare *ἐργασία*.

Greek *ἐσπερον* (θ. 529), Armenian *giri* 'slave'. The Greek word is not, therefore, to be connected with *servus*. See what was said above (p. 38) on the roots *ser-* and *ger-*.

Armenian *vaz* 'cursus' points to the root *veḡh*, for which *v* must therefore be assumed. Thus the future of (F)*έχω* would be *έξω*, and the correspondence with (σ)*έχω*, *έξω*, complete. From the same root appears to come Arm. *viz* 'collum'; compare Lat. *cer-vīc-*.

Armenian *vaḡ* 'pavor', Greek *ἄχος*. The derivation of the latter from *ak-* 'sharp', involved a very difficult aspiration of the guttural. The spiritus asper, representing the *v*, disappears by dissimilation, as possibly in *ἀπ-αρχ-ή*, compare Arm. *varj* 'usura'.

Armenian *gēž* 'hebes', Germ. *weich* (?).

Armenian *gowin* 'vas lapideum', Latin *urna*.

Armenian *gan* 'ulcus', Gothic *vunds*, Greek *ἀἶραι*, unless *vunds* is rather to be connected with *vnas* 'damnum'. Greek is indecisive, as the *ā-* is anaptyctic.

An interesting illustration is given by Brugmann's able conjecture¹ that Armenian *veç* 'six' rests upon an original form without initial *s-*. That *su-* (*sv-*) should become *v* in Armenian is unlikely as the examples *k'oir* (294) = *suesor-*, *k'irtn* (291)² = *suid-* are distinctly against it. By the rule, Greek *έξ* would be the final form both of *sveks* and of *veks*, but the latter form may be clearly traced. Original *su-* (*sv-*) passed³ through 'F into '. On the Tab. Heracl. this process has been completed, and accordingly we find *έκάσταν* (I. 115), &c. On the other hand we find *Φέξ* consistently written just as *Φίκατι*, and hence it is natural to infer that it rests upon *veks* not *sveks*.

¹ *Grds.* § 589. 3.

see ZDMG. 35. 170.

² Not = Zend *areθna* as de Lagarde,

³ Brgm. *Grds.* § 166.

Of course this does not assert that Greek did not possess also the form σFέξ, and hence Fίκατι ξέξ, Cauer² 296. 5, supports instead of overthrowing the inference. It may further be noted that the assumption of a stage in the language at which F=s_u had become *;*, but F= *u* remained, will explain why a good many words in Homer which had s_u show no trace of F: for instance, ξέξ itself appears not consistent (Knös p. 220), ὀράω (p. 43 *supr.*) shows no trace of it, from ἰδρῶς &c. it is also absent (Knös p. 223), whereas by the hypothesis proposed, *supr.* p. 32, ἰδος &c. ought to show F, but there is no decisive passage¹.

Note.—Another such doublet may be thought to give a better explanation of ἔλκω &c., than that on p. 36.

Thus s_uelq—Gk. ἔλκω, Lat. *sulcus*.

uelq—Lith. *velkū*, Lat. *ulcus*, Gk. αὔλαξ².

Then the absence of F from ἔλκω in Homer is to be explained on the lines just indicated.

It is possible that the same may be true for the Hesychian ξορ· θυγάτηρ, which is commonly identified with *svasar* &c. Then

s_uesor, Skt. *svasar*, Lat. *soror*,

uesor, Gk. ξορ-

but the source of the word is too completely unknown to make this even probable.

From the identification of Skt. *vrj* and Gk. εἴργω proposed above, it follows that I.Eu. v must be assumed for this root. The same holds for Skt. *varṣistha-* &c., from Fick's³ identification of it with Armenian *ver* (265), but in these cases no corroboration exists.

Perhaps the most startling result of the rule is the separation which it involves of *Eωs from *uśas*. The root ves 'shine', 'burn', which appears in Skt. as *vas-*, is proved to have had initial v (assuming for the present that my rule holds) by the Greek derivatives *Eωs, *Eσπερος, ἐστία (dissimilation in ἐσχάρα), and *perhaps* with Ascoli ἡμέρα (*supr.* p. 30). But

¹ Only ἰδιον, *Od.* xx. 204.

² Kuhn's *Beitr.* vii. p. 365, cf. ZDMG.

³ Referred to ἔλκω by Brgm. *Grds.*, 36. 121.

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the frequency with which *us*, *uchāmi* (i.e. *us-skhō*¹) &c., occur in Skt. would seem to point to the opposite conclusion. It is therefore necessary to separate *us* from *vas* and refer it as weak form to the root *aus* which appears in *αὔριον*, *Aurora*, Lesb. *αὔως*, Doric *Ἄως*, which cannot possibly represent **Eōs*².

For this root see Osthoff's excellent excursus, *Perfect* pp. 484—493, and his note on p. 135, which needs comment. He there separates, as I do, *vas* and *us*, but to the former he denies any existence, and refers *ἑστία* to a root *ues* 'dwell', Skt. *vas*, as opposed to *ves* 'clothe', Skt. *vas*. But that the list of words which he gives on p. 35 should all be analogical formations on *uvāsa*, when the latter owes its own existence to a difficult proportion, is in the last degree improbable. Of course the reference of *ἑστία* to *ues* would disprove my rule. My scheme is then as follows.

- | | | |
|----|--|----------------------|
| 1. | <i>ues</i> 'dwell', Skt. 5 <i>vas</i> , Gk. <i>φάστυ ἄστυ</i> , | Lat. <i>verna</i> . |
| 2. | <i>ves</i> 'clothe', Skt. 3 <i>vas</i> , Gk. <i>ἔννυμι εἶμα</i> &c., | Lat. <i>vestis</i> . |
| 3. | <i>ves</i> 'shine', Skt. 2 <i>vas</i> , Gk. <i>ἑστία Ἔως</i> , | Lat. <i>Vesta</i> . |

The derivation of *ἔαρ* from 2 *ves* as 'the clothier' must therefore be given up.

§ 9. In conclusion, the roots which assume the form *Feλ* in Greek (pp. 36–8) can now be reduced to a more systematic result. The presence or absence of the aspiration being admitted to serve as a guide to the original form, they may be tabulated as follows:—

1. *vel* 'burn' (perh. : *svel*), Gk. *ἔλη*, Arm. *var*.
2. *vel* 'press' 'surround', Gk. *ἄλις* &c., Skt. *vr vr̥ṇoti* (nos. 2, 3, 4, 7, 8 Grassmann).
3. *uel* 'circle' 'wrap', Gk. *εἶλω*, Lat. *volvo*, Arm. *gael*, Skt. *vr vr̥ṇoti* (nos. 1, 5, 6, 9 Grassmann).

The point of contact of roots 2 and 3 in Skt. may best be seen in no. 9 Grassmann ('to roll up tight') and their fusion probably explains the double type of stem, *vr̥ṇu-* and *ūr̥ṇu-*.

¹ Moulton, A. J. P. Vol. viii. (whole no. 30) p. 208.

² The Homeric form *ἠώς* has the prefix *ἦ-* mentioned supr. p. 37. Its flexion is therefore *ē-vōs(-s?)=ἠώς*,

gen. *ē-vōs-ōs=ἡφῶς ἡφῶς*. The stem of **Eōs* (note the difference of accent) is either *ves-es-* with formative suffix *-es* or a reduplicated one *ve-vōs-*.

It might seem tempting to derive ἄλσος from the third of these roots, which would give an excellent sense, but the entire absence of **F** in Homer precludes this. That it should equal *saltus* (p. 26 *supr.*) is quite improbable, but there seems no reason to question the old connexion with ἀλδαίνω, and so perhaps refer it to *alvus alveus* as 'the moist, fresh place'.

It is from the third of these roots that the explanation of ἄλσο, ἄλτο, ἄλμενος (*supr.* pp. 26, 29) comes. It is curious that the mistaken view of these as 'parts' of ἄλλομαι should have caused the evidence for the digamma to be so persistently ignored. It is true that in the frequent connexion σὺν τεύχεσι ἄλτο χαμᾶζε as well as in νευρὴ δὲ μέγ' ἴαχε ἄλτο δ' οἰστός Δ. 125, the insertion of paragogic ν is possible; but in A. 532, no such artifice is available. ἄλτο¹ is a regular unaugmented root-aorist, middle², of the root χελ-, χλ-, its exact analogues being χύτο, σύτο: ἄλμενος is the corresponding participle, and hence not to be separated from ἀλείς, as is commonly done. In the case of a root like χελ- of which the original sense appears very indeterminate, the senses developed are generally both numerous and varied, but fortunately, enough passages are found in Homer to enable the lines of development to be traced.

From the root meaning, that of 'collect' is easily deducible: hence its use in Π. 714,

ἧ λαοὺς ἐς τεῖχος ὁμοκλήσειε ἀλῆναι,

and Φ. 607,

ἀσπάσιοι προτὶ ἄστυ, πόλις δ' ἔμπλητο ἀλέντων,

and similarly in Φ. 534, X. 12.

In the singular, of course, the only possible corresponding sense is 'gather oneself together' and this it has in Φ. 571,

ὥς εἰπὼν Ἀχιλλῆα ἀλείς μένευ, ἐν δέ οἱ ἦτορ κτέ.

and in Π. 402,

ὁ μὲν εὐξέστω ἐνὶ δίφρῳ

ἦστο ἀλείς· ἐκ γὰρ πλήγη φρένας, ἐκ δ' ἄρα χειρῶν
ἥνία ἥιχθησαν.

¹ The accent ἄλτο rests on a mistaken belief that the augment was absorbed: compare also the analogy of

ἦστο πλήτο. The difficulty is felt by G. Meyer, *Gr. Gram.* § 530, p. 465.

² G. Meyer, l. c. §§ 525-7.

Not far different is the meaning in X. 308 (= ω. 538),

οἴμησεν δὲ ἀλείς, ὥστ' αἰετὸς ὑψιπετής.

When an eagle or other bird of prey swoops, it 'gathers itself together' by folding its wings, and hence the appropriateness of the word.

The next passage is Σ. 616,

ἡ δ' ἔρηξ ὥς ἄλτο κατ' Οὐλύμπου νιφόεντος
τεύχεα μαρμαίροντα παρ' Ἡφαίστοιο φέρουσα.

In this, the received view necessitates passing to a totally different root and sense, and speaking of the goddess as 'jumping', which is not poetical, and further, is not true of a hawk. The proper translation is 'swooped' and so also in A. 531,

ἡ μὲν ἔπειτα
εἰς ἄλα ἄλτο βαθείαν ἀπ' αἰγλήεντος Ὀλύμπου
Ζεὺς δὲ ἐὼν πρὸς δῶμα¹.

From this to the oft-recurring σὺν τεύχεσι ἄλτο χαμάζε involves a very slight change of meaning, but it is in this phrase that probably lies the cause of the derivation from ἄλλομαι.

There are three passages (M. 391, T. 353, Δ. 125) in which the sense is quite distinct, and as two of those reject the digamma while the third is doubtful, they may have to be referred to a different root.

§ 10. To sum up.

1. The spiritus asper is the regular representative in Greek of

- (a) Original s initial
- (b) Original v^a initial

¹ The unfitness of 'jump' as a translation in these two passages was pointed out long ago by Kuhn (K. Z. 5. 206-9). I cannot agree with him (and Fick, *Ilias*, p. 79) in finding difficult the ellipsis of the verb in the clause Ζεὺς δὲ κτέ. Zeus cannot be said to 'swoop' into his own house

any more than to 'jump', but a more general verb of motion can easily be supplied. ἄλτο itself tends to become more general. Cf. φ. 338.

² [For the value which Mr Darbishire ultimately attached to this symbol see the passage already referred to, p. 197 inf.]

- (c) Original $s\bar{u}(sv)$ initial
- (d) Original $\bar{i}$ initial
- (e) Original $s\bar{i}$ initial, and is
- (f) Hysterogene before Greek $\bar{u}$ initial.

2. Its phonetic characteristics are, dental in *a*) labial in *b*) and *c*) palatal in *d*) *e*) and *f*).

3. Apart from the working of the wider laws of analogy and dissimilation, exceptions to the rules are all doubtful.

4. Etymologies in which the spiritus asper cannot be accounted for by one of the above rules (or by analogy) are open to suspicion.

ADDENDA.

EXCURSUS I. On $s\mu$, $s\dot{\iota}$ (page 21, footnote 3).

THE treatment of the groups $s\mu$ - and $s\dot{\iota}$ - perhaps deserves a fuller explanation. The difficulty of the former when initial is well known, and it forms no part of my present subject to investigate those instances in which Greek represents it by σ . As stated in the text, the regular process, when initial, is for the s to assimilate the μ to its own breathed character and the μ to assimilate the s to a labial spirant, so that the result is $\dot{\iota}$ (breathed μ ,—initial sounds are not doubled). On the other hand, when medial, the retrospective assimilation of the μ to s , instead of being partial, is complete, and we get $\sigma\sigma$. The treatment of $s\dot{\iota}$ is analogous, though not identical. When initial, by mutual assimilation we get $\dot{\iota}\dot{\iota}$, and $\dot{\iota}$, whence ϵ . When medial, according to Brugmann (*Grundriss* I. p. 119, § 131), the $\dot{\iota}$ first suffers epenthesis and then the σ is lost. Thus, $to-s\dot{\iota}o$, $*\tau\sigma\iota\sigma o$, $\tau\sigma\dot{\iota}o$. But there is no ground for supposing that σ would be lost after $\dot{\iota}$ any more than before it, and Brugmann himself seems somewhat to have modified his views (cf. *Griech. Gram.* ed. 2, p. 29). What takes place is really this: $to-s\dot{\iota}o$ becomes $*to\dot{\iota}\dot{\iota}o$ and $to\dot{\iota}o$ by assimilation as above, and of this the Homeric $\tau\sigma\dot{\iota}o$ is the direct graphic representation. The further change to $\tau\acute{o}o$, Att. $\tau\sigma\dot{\upsilon}$, then requires no explanation. The procedure when a nasal precedes is different and has occasioned some discussion. The most important examples are $\nu\acute{\iota}\sigma\sigma o\mu\alpha\iota$ and $\pi\acute{\tau}\acute{\iota}\sigma\sigma o$, which according to Osthoff, *V. i. d. Nominalc.* 339 f., are for $*\nu\acute{\iota}-\nu\sigma-\dot{\iota}o-\mu\alpha\iota$ and $*\pi\tau\acute{\iota}\nu\sigma-\dot{\iota}o$ respectively. Wackernagel (*K. Z.* 29. 136) prefers $*\nu\acute{\iota}-\nu\sigma-o-\mu\alpha\iota$

for the former, remarking "dass *j* vorausgehendes *σ* zu schärfen vermocht habe, lässt sich nicht im geringsten wahrscheinlich machen. Wissen wir doch im Gegenteil dass intervocalisches *σj* wesentlich gleich behandelt wird wie intervocalisches *σ*." This reasoning is faulty in two respects: *sj* is not treated identically with *s*, although the final form is the same in both cases; and *sj* in *νινσιομαι* is not intervocal. Nor does Brugmann convince me (*Gr. Gram.*² p. 61) in postulating the following series of changes: **νινσιομαι* **νῖνσομαι* *νῖσομαι*, *νίσσομαι*, as the motive for the false analogy by which *σσ* replaces *σ*, which is weak in *πρίσσω*, is almost *nil* in the case of *νίσσομαι*, and Laroche's authority for *νῖσομαι* is by no means absolute. I see no difficulty in supposing that the retrospective assimilation which is regular for medial *sy* should not also act for *sj* when in a group of three. **νινσιομαι* then became *νίσσομαι* direct: it is not necessary to assume either **νινσσομαι*, *νίσσομαι* or **νινσομαι*, *νίσσομαι*, either of which would, I admit, be irregular.

EXCURSUS II. On *ῥ*- (page 24 f.).

I regret that I overlooked the attempted explanation of M. Henry (*Analogie*, p. 74), which renders necessary a fuller statement of the exact difficulties to be encountered. In the first place I must state his view and my reasons for disagreeing with it. He divides words with initial *ῥ*- into three groups: (1) those in which the aspiration is etymologically correct—nine in number; (2) those in which the *ν* represents the weak form of a root with initial *γ*—six in number; (3) those in which no such explanation exists. The second group, he thinks, show in the aspiration a reminiscence of the *F* of the strong form, and then these and the first group, fifteen in all, extend the aspiration by analogy to the other five. He has however underestimated the number of simple words beginning with *ῥ*-, of which there are 30 at fewest (see list below), and I cannot admit the existence of his second group, because, if the roots began with initial *γ*, it should regularly become the smooth breathing; if

on the other hand he supposes that they began with the spirant *v* and assumes that this becomes the rough breathing in Greek, then such roots would not show *u* in the reduced form. Further, it is impossible that analogy could have the influence he ascribes to it. Two words which have a very large amount of correspondence in form alone might conceivably without any correspondence in meaning produce a slight formal modification on each other, but that identity of the initial vowel should suffice is highly improbable. Numerical preponderance plays little or no part: on the one hand, all the words with initial *ῥ* have not influenced *ῥ*ᾱρος, on the other, compare the influence of *steti* in the formation of modern Italian perfects.

I believe M. Henry is right in seeking the explanation in analogy and not in any phonetic law. The analogy, however, is exerted through sense connexion and on individual words, not on an entire group through form alone. In our present state of uncertainty as to the derivations of many of the words in question, it is impossible to exhaust the subject, but the following is an outline of the method I would employ.

Neglecting one or two words of foreign origin we find:—

ῥαλος	ῥλάω	ῥπνον	ῥσκληος
ῥβός, ῥβρις	ῥλη	ῥπνος	ῥσμίνη
ῥγιής	ῥμείς	ῥπό, ῥψος	ῥσπληξ
ῥγρός	ῥμήν	ῥραξ	ῥσσακος
ῥδέω	ῥμνος	ῥράξ	ῥστέρα
ῥδωρ	ῥνις	ῥρισος, ῥρον	ῥστερος
ῥθλος	ῥπαρ	ῥρχη	ῥφή
ῥίός	ῥπέρ	ῥς	ῥω.

The aspiration is justified by etymology in

ῥίός, Skt. *sū-nú-* &c.

ῥλη, cf. Lat. *silva*.

ῥμνος, ῥμήν, Skt. *syūtá-* *syátman-* (see below).

ῥμείς, cf. Gothic *iūs*, Skt. *yusmá-*.

ῥπνος, Skt. *svápna-*.

ῥραξ, cf. Lat. *sorex*.

ῥς, Lat. *sus*.

ύσμίνη, Skt. *yudh*.

ύω, Skt. *su-*, the same root as in *sū-nū-*, the primitive meaning being 'to sprinkle'.

In *ύράξ* the existence of *σύρω*, in *ύρισος* (and *ύρον*) of the form *σύρισος*, renders the assumption of original *su-* probable.

Of *ύνις*, *ύπαρ*, *ύπνον*, *ύσκλος*, *ύσπληξ* the derivation is uncertain.

For the following words an etymology can be proposed which accounts for the aspiration.

ύλάω is probably connected with Skt. *svar*, 'be noisy': it is thus perhaps akin with *ύλη*, i.e. the place of noise or rustling.

ύπέρ and *ύπό* may be taken together. These two words are commonly separated from Lat. *super* and *sub* and equated with Skt. *upari* and *upa*. This is because the Latin words are supposed to be the reduced forms of e)xuper and e)xub. I prefer to analyse them *s-uper* and *s-up(o)*, making *s-* the prefix referred to below (Exc. III.) and tracing the Greek forms also back to the same.

ύστέρα is hardly to be connected with *uterus*. It is more probable that it contains the root *ḷu-* (?*ḷius-*) seen in Skt. *yóni-* (cf. *yóṣan-*). Its primary meaning seems to be 'encompass', 'contain'.

ύφή may be akin to the Skt. (*vā*) *u* and (*vabh*) *ubh*, but it is the exact phonetic equivalent of the Latin *iuba*, 'mane' (i.e. *ḷubhā*), and the sense suits admirably, 'tangle' accounting for both.

Lastly *ύαλος*, although the aspiration is accounted for if it be connected with *ύω* (?), may stand for *ḷuḷ-lo*² and so be connected with Lat. *iuvat*.

In all the remaining words the derivation goes to show that the breathing should be smooth. Thus

ύβός, *ύβρις* are probably connected with Skt. *oj*, *uj*, and therefore go back to the root *ug*. Cf. Brgm. *Grds.* § 430.

¹ I cannot feel sure that the root *ḷus-* must not also be postulated for *ύσμίνη*, for *dh* does not become *σ* before *μ*. It may also account for *ύσκλος* and *ύσπληξ*.

² It is curious how exactly this corresponds to the E. E. *iuvet*, but there seems no doubt that this was borrowed from the French.

Similarly *ύγής* is connected with Skt. *vaj-*, Lat. *vegeo*, and so has the $\sqrt{u\tilde{g}}$ (*Grds.* I. § 402).

ύγρός contains the root *ug* seen in Lat. *uvidus*.

ύδέω is explained by Skt. *vad* and so as *uad*: *ud*.

ύδωρ again contains *ud* (Skt. *ud-án*, Lat. *unda*, cf. 'Αλοσ-*ύδ-νη*) the reduced form of *uad* (Goth. *vatō*).

ύθλος is of doubtful etymology: it cannot be = *ύδ-λος* and so with *ύδέω*¹. I believe it to contain *u-*, the weak form of I. Eu. *uē-*, 'speak' (p. 27), with the suffix *-θλο-*, for which see Brgm. *Grds.* II. p. 201.

ύρχη is equated with Lat. *orca urceus*: there does not seem to be much authority for the aspiration in this word.

ύστερος, Skt. *uttara* (? *s-ud*: *ud*, as *s-upo*: *u-po*, &c.).

For the word *ύμήν* see *Classical Review*, Vol. IV. pp. 157, 273 [below, p. 62]. For the sense of 'membrane' a derivation is there proposed involving a smooth breathing. The two words *ύμήν* and *ύμήν* were then assimilated to one form (cf. *έλλός*, p. 36). The aspiration of *ύδέω* and *ύθλος* is most probably due to analogy from *ύμνος*. Similarly *ύγρός* and *ύδωρ* may be explained by the aspiration in *ύω*.

EXCURSUS III. On the prefix *s-* (page 32 § 3 ad fin.).

I am now inclined not to consider these as sentence doublets. It is true that, where the *s-* is followed by an explosive, it might disappear if the preceding word ended in certain other explosives, but such conditions would hardly occur frequently enough to account for the great prevalence of *s-less* forms, and further when the *s-* preceded a sound like *u*, it would not disappear even when an explosive preceded. Now many of the doublets do begin with *su* (cf. pp. 48, 49, 50, Brgm. *Grds.* II. p. 457). So also for (*s*)*l-*, for which see Brgm. in *Rh. Mus.* XLIII. p. 401, who postulates I. Eu. (*s*)*laiuο-* 'on analogy' of (*s*)*teg-*, &c., but the instances are too numerous. I regard this *s* element as a quasi-prepositional prefix or rather

¹ Brugmann (*Grds.* II. p. 202) connects it with *uēi*, but I cannot see the connexion in sense.

'element', and believe it also to be found in *s-super*, ὑπέρ, *upari*; *s-sub*, ὑπό, *upa*. It would be easy further to assert that it is the reduced form of I. Eu. *es* (? Gk. ἐς) and that it is found in ἐκ-ς, ἐν-ς, ἀκρι-ς, οὐτω-ς, &c., but this would take us back to a state of language so rudimentary as practically to be beyond our knowledge.

As Dr Brugmann (*Gram.*² pp. 30 and 65) thinks my examples insufficient to prove my case for I. Eu. *u*, *v*¹, I fear I have not made clear my general argument, which takes this line:

Greek,

ζέω, ζυγόν
ἄγιος, ὑμεῖς.

Latin,

j alike for both.

Sanskrit,

y alike for both,
but

Sanskrit has *yasta*)(*iṣṭa* where
Greek has ζέω, ἄγιος respec-
tively ;

therefore

Gk. ζ)(' represents an original
distinction between *j* and *i*.

Grundriss I. § 117.

Greek,

ἐννυμι, ἐκών, εἴλη, ἦλος, &c.
ἔπος, ἰδεῖν, ἔργον, οἶνος, &c.

Latin,

v alike for both.

Sanskrit,

v alike for both,
but

Sanskrit has *vavase*)(*ūce* where
Greek has ἐννυμι, ἔπος respec-
tively ;

Add to this that in Arme-
nian three roots with initial *v*
have been shown to have Greek
cognates: in *all* of these Greek
has the rough breathing: viz.
vasn ἐκών, *var* εἴλη, *vēlk'* ἦλος,
with possibly *vēm* αἰμύλος αἰ-
μασιύ. In all the roots where
Greek replaces **F** by ' Armenian
shows *g* initial ;

therefore

there is *more* evidence for an
original distinction here than
there is for *i*, *j*.

¹ [As before, see p. 197 inf.]

p. 25 § 3 a. G. Meyer, *Gram.*² p. 21, gives Fröhde's (B. B. 7. 85) equation of ἄσις with Lat. *sentina*. This would then be a violation of the rule. But ἄσις = ἡτι-s, which is the more primitive form of ἡτι- (Skt. *ātī-*, Greek *νήσσα*), so that the duck is the "marsh-bird".

νήσος may be connected with this root if *νησιώτης* be taken as proving that it stands for **νησις* on analogy of *χέρσος*.

p. 34, l. 14. I have here expressed myself carelessly¹. I did not intend to imply, as M. Henry seems to think, that *ἦθω* regains its aspiration from the etymology which I propose, but from the undoubted popular connexion with *σῆθω*.

p. 50, l. 15. I withdraw the comparison of Greek ἄστν and Latin *verna*. M. Henry is right in denying that it is possible to derive the former from √ *ues* in the present state¹ of our knowledge.

I have to acknowledge my indebtedness to M. V. Henry in the *Revue Critique*, 1889, p. 383 f., and to Dr Brugmann in the 2nd ed. of his *Griechische Grammatik* for their criticisms and remarks on my essay, of which I have gladly availed myself.

¹ [I leave these two paragraphs in the form in which they appeared, as a reminiscence of Mr Darbishire's most characteristic frankness. The sentence on p. 34 (now corrected) originally ran: 'So ἦθω loses its aspiration by the same law and regains it from its etymology.' (Even

with the correction, I confess, the explanation is not quite clear to me, but I did not feel justified in omitting the word altogether.)

In ἄστν I know that Mr Darbishire subsequently recognised an *ə* of the *ē*-series with Bartholomae. C.]

(4) ON THE DERIVATION OF LAT. *ŌMENTUM*.

(*Classical Review* iv, 1890 June p. 273.)

LAT. *ŌMENTUM*.

THE conclusions arrived at by Mr Still in his paper (*Class. Rev.* iv. (1890) p. 157) on the meaning and use of this word will without doubt be generally accepted. The derivation which he prefers is however open to objection. In the first place the correct equivalent of I. Eu. *pm* in Latin is, not a single *m* with compensatory lengthening, but *mm*, e.g. *summus* = sup-mo-s. This indeed is not insuperable, as, if we admitted *rūmentum* from √ rup, *āmentum* from √ ap, and *ōmentum* from √ op, we might reverse the rule, and explain *summus* like *Iuppiter*. The second objection is more serious. The termination *mn-to-* is very frequent both in Latin and in Greek¹, and its function is fairly clear. It is a secondary participial formation, prevailing with passive force, and consequently attaches regularly to verbal roots. From Greek, *κασύματα* and *δέγματα* are good examples. In Latin, I think, although it is very widespread, it occurs with verbal roots only, *alimenta*, *armenta*, *caementa*, *cognōmentum*, *pigmentum*, *sarmenta*, *tormentum*, and from secondary verbal stems *armāmenta*, *fundāmenta*, *vestīmentum* being a few of the commonest: *op* not being a verbal root in Latin, the derivation from it is therefore without precedent.

Mr Still does not refer to the most modern etymology—that of Windisch², who refers it to a root *uep*, Skt. *vapā*. This is also open to objection, and is marked as doubtful by Stolz³.

The other derivation (Vaniček's), of which Mr Still approves, is really preferable, although Byrne's principles may not be

¹ Brgm. *Grds.* II. § 82.

³ Müller's *Handb.* II.² p. 257.

² *Lit. Centralbl.* 1888, col. 668.

deemed a decisive proof. The root found in *induo* has really a more widespread existence than is generally recognized. In the modern orthography its forms are *eu*, *ou*, *u*: from the first form come *induō*, *exuō*, through *-ouō* from *-euō*; from the second (probably) Umbrian *anovihimu* (*Tab. Ig. VI. B. 49*); from the third *exuviae*. This however does not exhaust the root; *ōmentum* appears to come from the *eu*- stage (cf. *caementum*), as instances of *o*- Ablaut with this suffix seem not to occur. The primary meaning of the root seems to be 'draw', and hence it provides a satisfactory derivation for *rūmen*¹, i.e. *re-u-mēn*, cf. Serv. *Verg. Ecl. 6. 54*, who defines it² as *eminens gutturis pars per quam demissus cibus a certis reuocatur animalibus*. Again, to this root may be referred Latin *αιεō* and Skt. *av*³. The Latin verb is causative and so the root-form would be *ou* (like *moneō*, *φορέω*), **ouēō* would become *auēo* by Thurneysen's rule⁴. The development of sense is of course very simple (cf. *attractive*), and appears also in Sanskrit.

Finally I would remark that Mr Still's reference of *ύμήν* to this root is, so far as I know, new. The accepted derivation⁵ is from *√ sinu*, Lat. *suo*, which I think is distinctly inferior. The only meaning of the word which it suits is that of marriage (*Υμήν, ύμεναίος* &c.) and even for these the sense of 'veiling' given by our root accords much better with all that we know about the Indo-European view of the matter.

¹ Other etymologies: (a) *√ sru* Curt. *Grdz.*⁵ 353-4; (b) *√ rug*, Kluge, *Etym. Wört.* ed. 4, p. 274^b. It might also be possible to establish a connexion with Greek *έρω*.

² There is a variant *ruma*.

³ Generally equated with the supposed Greek *άω*. This I have endeavoured

to prove non-existent (*Trans. Camb. Phil. Soc.* III. 94 [p. 38 above]).

⁴ [I venture to doubt if Mr Darbishire retained this belief in Thurneysen's theory, though I can recall no direct utterance of his on the matter. C.]

⁵ This originated, I think, with Pott.



(5) ON THE MEANING AND USE OF

ἐπιδέξιος ἐπιδέξια, ἐνδέξιος ἐνδέξια.

(Read before the Cambridge Philological Society on March 4, 1890,
and printed in the *Cambridge Philological Transactions*, Vol. III.
p. 154.)

This paper was to have been the first of a projected series of 'Contributions to Greek Lexicography'.

Ἐπιδέξιος ἐπιδέξια, ἐνδέξιος ἐνδέξια.

THESE words cannot be satisfactorily discussed without touching upon δεξιός, and the most convenient starting-point is to begin by quoting Liddell and Scott's account of all three words.

ἐπιδέξιος, ov, towards the right, i.e. from left to right:
 I. used by Hom. only in neut. pl. as Adv.; ὄρνυσθ' ἐξείης ἐπιδέξια rise in order *beginning with the left hand man*, as the wine is served, Od. 21. 141, Plat. Symp. 214 B; πίνειν τὴν ἐπιδέξια (sc. κύλικα) Eupol. Incert. 33, cf. Anaxandr. Ἀγρ. 1, Ath. 463 F sq.: hence *auspicious, lucky, ἀστυράπτων ἐπιδέξια* (explained by the next words, ἐναίσιμα σήματα φαίνων) Il. 2. 353; ἐπιδέξια χειρὸς Pind. P. 6. 19, Theocr. 25. 18:—when strongly opposed to *the left*, it was written ἐπὶ δεξιὰ opp. to ἐπ' ἀριστερά Il. 7. 238, Plat. Theaet. 175 E; but τὰ ἐπιδέξια opp. to τὰ ἐπαρίστερα Hdt. 2. 93, cf. 4. 191, 6. 33. 2. after Hom. the sense of *motion towards* died away (cf. however Ar. Pax 957) and the word became = δεξιός *on the right hand*, Xen. An. 6. 2. 1, etc.; τὰ ἐπιδέξια *the right side*, Ar. Av. 1493. II. as Adj., of persons, *dexterous, capable, able, clever*, Aeschin. 25. 21, Arist. Eth. Nic. 4. 8. 5; c. inf. *clever at doing*, Id. Rhet. 2. 4. 13; ἐπ. πρὸς τι Polyb. 5. 39. 6; περὶ τι Plut. Aemil. 37:—as Adv. ἐπιδέξια *dexterously, cleverly*, Anaxandr. Incert. 2, Nicom. Εἰλειθ. 1. 27, Plat. Rep. 420 E; and -ως Polyb. 3. 19. 13, 4. 35. 7, etc. 2. *lucky, prosperous, τύχη*, Diod. Exc. Vat. p. 5. Cf. ἐνδέξιος.

ἐνδέξιος, α, ον: Homer has only the neut. pl. ἐνδέξια, towards the right hand, from left to right, mostly as Adv. θεοῖς ἐνδέξια πᾶσιν ὄνοχόει, he filled for all the gods from left to right, Il. 1. 597; δειξ' ἐνδέξια πᾶσιν Il. 7. 184; βῆ δ' ἵμεν αἰτήσων ἐνδέξια φῶτα ἕκαστον Od. 17. 365. The contrary procedure was avoided as *unlucky* (as in Iceland *ganga andsaelis*, Scott. *to go widdershins*, i.e. against the course of the sun, from right to left, v. Jamieson s.v.), cf. δεξιός; hence ἐνδέξια σήματα propitious omens, Il. 9. 236: cf. ἐπιδέξιος. 2. after Hom. without any sense of motion = δεξιός on the right, Eur. Hipp. 1360, ἐνδέξιος σῶ ποδί on thy right, Id. Cycl. 6.

II. as Adj. *clever, expert*, h. Hom. Merc. 454.—Ep. word, also in Eur. Il. c., but never in prose, for in Thuc. 1. 24 etc., ἐν δεξιᾷ is now restored, as opp. to ἐν ἀριστερᾷ.

δεξιός, á, óν, on the right hand or side, opp. to ἀριστερός, δ. μαζός, γλοντός, etc., Hom. etc.; τὸ δ. (sc. κέρας) the right of an army, Xen. Ages. 2. 9, etc.; cf. δεξίτερος:—often in adverb. usages, ἐπὶ δεξιᾷ on the right, like the Att. ἐν δεξιᾷ (v. sub δεξιᾷ), Il. 7. 238, etc.; ἐπὶ δεξιόφιν (Ep. gen.) towards the right, 13. 308; later also χειρὸς εἰς τὰ δεξιᾷ Soph. Fr. 527; ἐπὶ δ. χειρός Theocr. 25. 18 (as ἐπ' ἀριστερὰ χειρός in Hom.); πρὸς δεξιᾷ Hdt. 1. 51, 7. 69; cf. omnino δεξιᾷ. II. *fortunate, boding good*, esp. of the flight of birds and other omens, δεξιὸς ὄρνις = αἴσιος, often in Hom.—This sense came from the practice of the Greek augurs, who always looked to the North, so that *lucky* omens which came from the East were on the right, while the *unlucky* ones from the West were on the left. To the Romans, on the contrary, who looked South (Liv. 1. 18) the good omens came from the left (*laeva prospera existimantur* Plin. 2. 55); but the poets mostly followed the Greek usage, v. Coningt. Virg. G. 4. 7. From the Greek preference of the right hand, it was considered lucky to hand wine from left to right, Il. 1. 597; so also in handing round lots, begging round a table, cf. 7. 184, Od. 17. 365, 21. 141, Theogn. 938; v. ἐνδέξιος, ἐπιδέξιος. III. metaph. *dexterous, ready*,

opp. to *σκαίος* (sinister, French *gauche*); and of the mind, *sharp, shrewd, clever*, first in Pind. I. 5. 77 (4. 61), who has also Sup. in this sense, N. 3. 12; then freq. in Ar., both of persons and things, as Nub. 428, 834; also in Prose, Thuc. 3. 82, etc.; *δεξιὸν ποιεῖν* a *clever thing*, Antipho 113. 26; *Εὐριπίδου δράμα δεξιώτατον* Strattis 'Ανθρ. 1; *δ. περί τι* Plat. Hipparch. 225 c:—Adv. *δεξιῶς* Antiph. Incert. 5, etc.; Sup. *δεξιώτατα* Ar. Nub. 148.

The facts are by no means so simple as they at first sight appear, and to obtain a clear view of the subject the history of 'right' v. 'left' must be briefly sketched.

The distinction between 'right' and 'left' and the superiority of the former arise from natural causes. The distinction itself appears to have preceded the consciousness of the distinction, and hence the first conception of the right hand was also the first perception of its importance. *A priori* then the right hand is that which is to be preferred for all purposes, and the first object of the present paper is to discover how far this conclusion agrees with the use of the words in the Greek and Latin authors. Here we are at once met by an apparent inconsistency. It is generally asserted that the Greeks regarded the *right* and the Romans the *left* as the lucky side, for which the great authority is Cicero *De Div.* II. 39, 82, *Ita nobis sinistra uidentur, Grauis et barbaris dextra, meliora*. So for example Plautus *Pseud.* 761, (cf. *Epid.* 182), has

Omnes ordines sub signis ducam, legiones meas
aue sinistra, auspicio liquido atque ex sententia.

The contrary use, which is distinctly later, is generally explained as due to Greek influence. Ovid has *auibus sinistris*, *Her.* 2. 115, Virgil has *dexter adi*, *Aen.* 8. 802, and see generally for examples Bulenger in *Graevii Thes. Ant. Rom.* v. p. 407 f.

It thus appears that to the Romans the left was the lucky side in opposition to the Greek usage and, one may add, to expectation. The reason for this inconsistency is deliberated on by Cicero in the passage already quoted and he concludes *Sed certe nostri sinistrum nominauerunt externique dextrum*

*quia plerumque melius id uidebatur*¹. Plutarch also debates the question *Quaest. Rom.* 78.

“Διὰ τί τῶν οἰωνῶν ὁ καλούμενος ἀριστερὸς αἴσιος;”
 πότερον οὐκ ἔστι τοῦτο ἀληθές, ἀλλὰ παρακρούεται πολλοὺς
 ἢ διάλεκτος; τὸ γὰρ ἀριστερὸν σίνιστρον ὀνομάζουσι, τὸ δ’
 ἀφεῖναι σίνερε, καὶ σίνε λέγουσιν, ὅταν ἀφεῖναι παρακαλῶσι.
 τὸν οὖν ἐφίεντα τὴν πρᾶξιν οἰωνόν, σινιστέριον ὄντα σίνι-
 στρον οὐκ ὀρθῶς ὑπολαμβάνουσιν οἱ πολλοὶ καὶ ὀνομάζουσιν.
 ἢ καθάπερ Διονυσίος φησιν Ἀσκανίῳ νύμφῃ Αἰνείου παρα-
 ταττομένῳ πρὸς Μεξέντιον, ἀστραπῆς ἐν ἀριστερᾷ νικηφόρου
 γενομένης οἰωνισάμενοι καὶ πρὸς τὸ λοιπὸν οὕτω παραφυ-
 λάττουσιν; ἢ, ὥς ἄλλοι τινές, Αἰνεία τούτου συμπεσόντος;
 καὶ γὰρ Θηβαῖοι τῷ ἀριστερῷ κέρατι τρεψάμενοι τοὺς πολε-
 μίους καὶ κρατήσαντες ἐν Λεύκτροις διετέλεσαν ἐν πάσαις
 ταῖς μάχαις τῷ ἀριστερῷ τὴν ἡγεμονίαν ἀποδιδόντες. ἢ
 μάλλον, ὥς Ἰόβας φησὶ, τοῖς πρὸς τὰς ἀνατολὰς ἀποβλέπ-
 ουσιν ἐν ἀριστερᾷ γίνεται τὸ βόρειον; ὃ δὴ τοῦ κόσμου δεξιὸν
 ἔνιοι τίθενται καὶ καθυπέρτερον. ὅρα δὲ μὴ φύσει τοῖς εὐ-
 ωνύμοις ἀσθενεστέροις οὖσιν οἱ παριστάμενοι τῶν οἰωνῶν οἶον
 ἀναρρωννύουσι καὶ ὑπερείδουσι τὸ ἐλλιπές τῆς δυνάμεως ἐπ-
 ανισοῦντες. ἢ τὰ ἐπίγεια καὶ θνητὰ τοῖς οὐρανίοις καὶ θεοῖς
 ἀντικεῖσθαι νομίζοντες ᾤοντο τὰ πρὸς ἡμᾶς ἀριστερὰ τοὺς
 θεοὺς ἀπὸ τῶν δεξιῶν προπέμπειν;

The generally accepted explanation is that the Roman augurs looked to the south, the Greeks to the north: in each case the East was the auspicious quarter: therefore to the former the left hand, to the latter the right, was the favourable side.

It may be noted that, except in the language of augury, the Roman diction does not differ from that of other peoples. The metaphorical use of *dexter* is ‘clever’; of *sinister* and still more of *scaevus* and *laevus* is ‘stupid,’ ‘unskilful,’ ‘perverse.’ Nay, although *sinister* = ‘favourable’ is an early use, we never find the corresponding use of *dexter* as ‘unfavourable.’

¹ The reading of the passage is not quite certain—*melius indiuidebatur*, cod. Paris., unde Dav. coni. *meliora inde uidebantur*.

Turning for a moment to the Greek side of the question our natural starting-point is M. 237,

τὴν δ' οἰωνοῖσι τανυπτερύγεσσι κελεύεις
 πείθεσθαι, τῶν οὔτι μετατρέπομ' οὐδ' ἀλεγίζω,
 εἴτ' ἐπὶ δεξι' ἴωσι πρὸς ἡὼ τ' ἡέλιόν τε,
 εἴτ' ἐπ' ἀριστερὰ τοίγε ποτὶ ζόφον ἡερόεντα,

where it is clear that the place in which the birds appear is not so important as the direction of their flight¹. We may accordingly draw the same conclusion for the lightning flash in B. 353,

ἀστράπτων ἐπιδέξι' ἐναίσιμα σήματα φαίνων,
 and I. 236,

Ζεὺς δέ σφι Κρονίδης ἐνδέξια σήματα φαίνων
 ἀστράπτει,

that is to say in both these cases the flash, like the flight of the birds, is from left to right, and the interpretation which makes ἐπιδέξια (ἐνδ.) signify 'on the right hand' is wrong². The early Roman view is precisely similar. Dionysius Halicarn. II. 5 has the following important passage.

μετὰ δὲ τὴν εὐχὴν ἀστραπὴ διήλθεν ἐκ τῶν ἀριστερῶν ἐπὶ τὰ δεξιὰ. τίθενται δὲ Ῥωμαῖοι τὰς ἐκ τῶν ἀριστερῶν ἐπὶ τὰ δεξιὰ ἀστραπὰς αἰσίους, εἴτε παρὰ Τυρρηνῶν διδαχθέντες, εἴτε τῶν πατέρων καθηγησαμένων κατὰ τοιούδε τινα ὡς ἐγὼ πείθομαι λογισμὸν, ὅτι καθέδρα μὲν ἐστὶ καὶ στάσις ἀρίστη τῶν οἰωνοῖς μαντευομένων ἢ βλέπουσα πρὸς ἀνατολὰς, ὅθεν ἡλίου τ' ἀναφοραὶ γίνονται καὶ σελήνης καὶ ἀστέρων πλανήτων τε καὶ ἀπλανῶν· ἢ τε τοῦ κόσμου περιφορὰ δι' ἣν τοτὲ μὲν ὑπὲρ γῆς ἅπαντα τὰ ἐν αὐτῷ γίνονται, τοτὲ δ' ὑπὸ γῆς, ἐκείθεν ἀρξαμένη τὴν ἐγκύκλιον ἀποδίδωσι κίνησιν. τοῖς δὲ πρὸς ἀνατολὰς βλέπουσιν ἀριστερὰ μὲν γίνονται τὰ πρὸς τὴν ἄρκτον ἐπιστρέφοντα μέρη, δεξιὰ δὲ τὰ πρὸς μεσημβρίαν φέροντα· τιμιώτερα δὲ τὰ πρότερα πέφυκεν εἶναι τῶν

¹ So in CIG 2953 the flight from left to right is to be observed; the omen however is then judged from the motion of the wings.

² This is probably due to a belief

that ἐνδέξια was a dative compound = ἐν δεξιᾷ. It is here however ἐν with the accusative (as in Pindar and in Latin), which was later supplanted by ἐς.

ὑστέρων. μετεωρίζεται γὰρ ἀπὸ τῶν βορείων μερῶν ὁ τοῦ ἄξονος πόλος περὶ ὃν ἡ τοῦ κόσμου στροφή γίνεται· καὶ τῶν πέντε κύκλων τῶν διεζωκότων τὴν σφαῖραν ὁ καλούμενος ἀρκτικός αἰεὶ τῇδε φανερός, ταπεινοῦται δ' ἀπὸ τῶν νοτίων ὁ καλούμενος ἀνταρκτικός κύκλος ἀφανὴς κατὰ τοῦτο τὸ μέρος. εἰκὸς δὴ κράτιστα τῶν οὐρανίων καὶ μεταρσίων σημείων ὑπάρχειν ὅς' ἐκ τοῦ κρατίστου γίνεται μέρους· ἐπειδὴ δὲ τὰ μὲν ἐστραμμένα πρὸς τὰς ἀνατολὰς ἡγεμονικωτέραν μοῖραν ἔχει τῶν προσεσπερίων, αὐτῶν δέ γε τῶν ἀνατολικῶν ὑψηλότερα τὰ βόρεια τῶν νοτίων ταῦτ' ἂν εἴη κράτιστα. ὥς δέ τινες ἰστοροῦσιν ἐκ παλαιοῦ τε καὶ πρὶν ἢ παρὰ Τυρρηνῶν μαθεῖν τοῖς Ῥωμαίων προγόνοις αἰσιοὶ ἐνομίζοντο αἱ ἐκ τῶν ἀριστερῶν ἀστραπαί. Ἀσκανίῳ γὰρ τῷ ἐξ Αἰνείου γεγονότι, καθ' ὃν χρόνον ὑπὸ Τυρρηνῶν, οὓς ἡγε βασιλεὺς Μεσέντιος, ἐπολεμεῖτο καὶ τειχέρης ἦν, περὶ τὴν τελευταίαν ἔξοδον, ἣν ἀπεγνωκὼς ἤδη τῶν πραγμάτων ἔμελλε ποιῆσθαι, μετ' ὀλοφυρμοῦ τὸν τε Δία καὶ τοὺς ἄλλους αἰτουμένῳ θεοὺς αἷσια σημεία τῆς ἐξόδου δοῦναι φασὶν αἰθρίας οὐσης ἐκ τῶν ἀριστερῶν ἀστράψαι τὸν οὐρανόν. τοῦ δ' ἀγῶνος ἐκείνου λαβόντος τὸ κράτιστον τέλος διαμεῖναι παρὰ τοῖς ἐκγόνοις αὐτοῦ νομιζόμενον αἷσιον τότε τὸ σημεῖον.

These passages make it clear that to both nations alike the right was primitively auspicious, and consequently signs proceeding from left to right were favourable omens. From this the Romans by an easy development came to regard mere appearance on the left as favourable, independently of direction, and so *sinister* acquired a certain limited acceptation as 'auspicious.' The Greeks however never lost sight of the primitive idea, and, as we have seen, the Romans never carried it so far as to give *dexter* the opposite signification, and were quite ready to restore the earlier acceptation as soon as Greek influence began to be felt. Hence Ovid could even say (*Ibis* 127)

dedit ipse mihi modo signa futuri
Phoebus et a laeua maesta uolavit ausis.

For the individual observer, then, auspicious signs were those on the *right* hand, or which travelled from left to *right*. But when attempts began to be made to ascertain the will of

the gods in some more formal and definite way than by such accidental manifestations it became necessary to fix the observer's position, or in other words to import an absolute right and left into Nature. The difficulty of this is evident from the divergence of the results, and the reason of this difficulty is twofold: first, either the East or the South may be regarded as the source of light, and consequently the observer may face either of these quarters; secondly, when his position is fixed, the universe may or may not be considered as facing him.

The Oriental view personified the universe, and selected the East as the quarter which should be faced. Hence the north becomes the absolute Right, the south the Left. So Plutarch *De Is. et Os.* 32 καὶ θρηνός ἐστιν ἱερός ἐπὶ τοῦ Κρόνου γεγόμενος, θρηνεῖ δὲ τὸν ἐν τοῖς ἀριστεροῖς γεγόμενον μέρεσιν, ἐν δὲ τοῖς δεξιοῖς φθειρόμενον. Αἰγύπτιοι γὰρ οἴονται τὰ μὲν ἑῶα τοῦ κόσμου πρόσωπον εἶναι, τὰ δὲ πρὸς βορρᾶν δεξιά, τὰ δὲ πρὸς νότον ἀριστερά. φερόμενος οὖν ἐκ τῶν νοτίων ὁ Νεῖλος, ἐν δὲ τοῖς βορείοις ὑπὸ τῆς θαλάσσης καταναλισκόμενος, εἰκότως λέγεται τὴν μὲν γένεσιν ἐν τοῖς ἀριστεροῖς ἔχειν τὴν δὲ φθορὰν ἐν τοῖς δεξιοῖς.

The same view¹ was taken by the Hebrews and by other Oriental nations and is probably a relic of sun-worship. As much of the Roman system of augury was undoubtedly influenced by Chaldaean astrologers, it is not surprising to find it clearly stated in the above passage from Dionysius, and again in Livy I. 18, [*augur*] *regiones ab oriente ad occasum determinavit dexter ad meridiem partes, laevas ad septentrionem esse dixit*, and Servius on Verg. *Aen.* II. 693, *sinistras autem partes septentrionales esse augurum disciplina consentit et ideo ex ipsa parte significatiores esse fulmina quoniam altiora et uiciniora domicilio Iouis*.

On the other hand the Greek philosophers from their *a priori* view of nature argued that the East must be the absolute Right because the motion of the heavenly bodies began thence.

The most important passage in this connection is Aristotle *De Caelo* II. cap. 2. The whole chapter is interesting and in-

¹ [i.e. the view based on the East-ward position of the observer, not that based on personifying the East. C.]

structive but too long for quotation. The course of the argument is briefly this. "We have three pairs of opposites, up and down, backwards and forwards, right and left. Motion upwards is growth, motion forwards is perception, motion right and left motion in space. Therefore everything that has motion in space must have a right and left, but the universe has motion in space and therefore has an absolute right and left." Now follows (p. 285 b 8) the most remarkable statement. λέγω δὲ μῆκος μὲν αὐτοῦ τὸ κατὰ τοὺς πόλους διάστημα καὶ τῶν πόλων τὸν μὲν ἄνω τὸν δὲ κάτω...τῶν δὲ πόλων ὁ μὲν ὑπὲρ ἡμᾶς φαινόμενος τὸ κάτω μέρος ἐστίν, ὁ δ' ἡμῖν ἄδηλος τὸ ἄνω. δεξιὸν γὰρ ἐκάστου λέγομεν, ὅθεν ἡ ἀρχὴ τῆς κατὰ τόπον κινήσεως· τοῦ δ' οὐρανοῦ ἀρχὴν τῆς περιφορᾶς, ὅθεν αἱ ἀνατολαὶ τῶν ἀστρων, ὥστε τοῦτ' ἂν εἴη δεξιόν, οὐ δ' αἱ δύσεις ἀριστερόν. εἰ οὖν ἀρχεται τε ἀπὸ τῶν δεξιῶν καὶ ἐπὶ τὰ δεξιά περιφέρεται, ἀνάγκη τὸ ἄνω εἶναι τὸν ἀφανῆ πόλον· εἰ γὰρ ἔσται ὁ φανερός, ἐπ' ἀριστερὰ ἔσται ἡ κίνησις ὅπερ οὐ φαμεν. Here in the first place it is clear that as the universe is regarded as a sphere there is no question of a subjective right and left facing and opposite to ours, as he himself remarks above (p. 285 a 1) ἀλλ' ἐν μὲν τούτοις λέγομεν τὸ ἄνω καὶ κάτω καὶ τὸ δεξιὸν καὶ ἀριστερόν πρὸς ἡμᾶς ἐπαναφέροντες· ἡ γὰρ κατὰ τὰ ἡμέτερα δεξιά, ὥσπερ οἱ μάντις, ἡ καθ' ὁμοιότητα τοῖς ἡμετέροις, οἷον τὰ τοῦ ἀνδριάντος, ἡ τὰ ἐναντίως ἔχοντα τῇ θέσει, δεξιὸν μὲν τὸ κατὰ τὸ ἡμέτερον ἀριστερόν, ἀριστερόν δὲ τὸ κατὰ τὸ ἡμέτερον δεξιόν κ.τ.λ. where the words ὥσπερ οἱ μάντις are especially important as shewing that in augury the heavens were not considered as *facing* the observer. It also seems clear from Aristotle's words that the common statement¹ about Greek augury, viz. that the observer faced the North, is inaccurate. Aristotle's difficulty that the East is *not* on our right hand proves that, as might be expected, he regards the natural position for observing the heavens to be facing the South. One point alone may be thought obscure. If the East be the left hand and the West the right, surely, although the heavenly bodies cannot be said *ἀρχεσθαι ἀπὸ τῶν δεξιῶν*, they do proceed

¹ e.g. Voss, *Krit. Bl.* 1. 42.

ἐπὶ τὰ δεξιά, and hence his argument about the South Pole breaks down. The explanation is that Aristotle is speaking not only of the visible but also of the invisible course of the heavenly bodies: he regards the point of time at which they rise for Greece as the beginning and consequently as the end of their diurnal course, and thus they begin from the extreme left and circle round to it again ἀπὸ τῶν ἀριστερῶν ἐπὶ τὰ ἀριστερά. Of course Aristotle believed that the sun rose on the Southern hemisphere as he set in this.

illi cum uideant solem nos sidera noctis
cernere et alternis nobiscum tempora caeli
diuidere et noctes parilis agitare diebus.

Thus our West is their East, and accordingly to *them* the diurnal motion begins on the extreme right and returns to it again¹. It is interesting to notice that the words τοῦ δ' οὐρανοῦ ἀρχὴν τῆς περιφορᾶς, ὅθεν αἱ ἀνατολαὶ τῶν ἀστρων, ὥστε τοῦτ' ἂν εἴη δεξιόν, from which Aristotle deduces the conclusion that the *absolute* rising of the sun is when it sets for us, have been taken apart from their context, so that Plutarch says (*Plac. Philos.* II. 10)

Πυθαγόρας, Πλάτων², Ἀριστοτέλης, δεξιά τοῦ κόσμου τὰ ἀνατολικά μέρη, ἀφ' ὧν ἡ ἀρχὴ τῆς κινήσεως, ἀριστερά δὲ τὰ δυτικά.

We thus have to reckon with two fundamental views concerning the observation of the heavens—the one which faced the East and the other which faced the South. It is this double possibility which is alluded to in Artemidorus (*Ὀνειροκρ.* II. 36, p. 130)

Ἴρις δὲ δεξιά μὲν ὁρωμένη ἀγαθὴ, εὐώνυμος δὲ πονηρά. δεξιὰν δὲ καὶ εὐώνυμον οὐ πρὸς τὸν οὐρανὸν χρὴ νοεῖν, ἀλλὰ πρὸς τὸν ἥλιον.

¹ It would perhaps be more correct to say that to *them* the heavenly bodies rise in the West, as Aristotle seems to regard the two points as no less fixed than North and South. This also explains how the elevation of the

South Pole does not affect his argument.

² Probably relying on *Legg.* vi. p. 760 D. On the contrary Plato says the diurnal motion proceeds ἐπὶ δεξιά, see *Tim.* p. 36 C.

That is, in interpreting signs from the rainbow the East and West are to be considered left and right (πρὸς τὸν ἥλιον) and not the North and South (πρὸς τὸν οὐρανόν); the reason is obvious:—a rainbow must always be opposite the sun and hence in the northern hemisphere can never appear in the South.

To return to our passage from Homer:

εἴτ' ἐπὶ δεξι' ἴωσι πρὸς ἡῶ τ' ἡέλιόν τε,
εἴτ' ἐπ' ἀριστερὰ τοίγε ποτὶ ζόφον ἡερόεντα.

This surely proves that the East was the right hand and the West the left for the purpose of augury and consequently that the observer must have faced the North. Before admitting this however other passages in Homer which are in point must be examined.

ζόφος is a word of uncertain etymology, of which the earliest traceable sense is "darkness" and "the underworld." Thus ὑπὸ ζόφον ἡερόεντα Ψ. 51, λ. 57, 155, ὑπὸ ζόφου ἡερόεντος of the slain Trojans Φ. 56. It is coupled with Erebus, *ἰευένων Ἐρεβόςδε ὑπὸ ζόφον* υ. 356, cf. μ. 81, and is the domain of Hades O. 191. Thus it does not in itself denote a point of the compass, but the realm of darkness which apparently extended from West to East below the earth. It is thither that the light of day departs γ. 335.

There are two passages which enable us to fix its meaning when contrasted with ἡῶ τ' ἡέλιόν τε. These are, first μ. 80 (the description of Scylla's cave).

μέσσω δ' ἐν σκοπέλῳ ἐστὶ σπέος ἡεροειδές,
πρὸς ζόφον εἰς Ἐρεβος τετραμμένον.

If we may trust the geography assigned to the legend ever since Thucydides (cf. iv. 24), the rock of Scylla lay on the Italian coast just outside the Straits of Messina. At this place, however, the coast-line runs nearly East and West, and the rock faces northwards, consequently the cave itself must have faced almost due North. The second passage gives the same conclusion. In ι. 22, describing Ithaca, he says:

ἀμφὶ δὲ νῆσοι
πολλαὶ ναιετάουσι μάλα σχεδὸν ἀλλήλησιν

Δουλίχιόν τε Σάμη τε καὶ ὑλήεσσα Ζάκυνθος.
 αὐτὴ δὲ χθαμαλὴ πανυπερτάτῃ εἰν ἀλλὶ κεῖται
 πρὸς ζόφον—αἱ δὲ τ' ἀνευθε πρὸς ἡῶ τ' ἡέλιόν τε.

Cephalonia and Zacynthus lie due South of Ithaca—the position of Dulichium is not certain, but if it was one of the Echinades it lay to the South-east; thus the widest interpretation that can be given to πρὸς ζόφον is N.W. to N., and of πρὸς ἡῶ κτλ. is S.E. to S., so that these two decisive passages are absolutely consistent¹.

In two other passages the expressions are opposed.

ἡμὲν ὅσοι ναίουσι πρὸς ἡῶ τ' ἡέλιόν τε
 ἡδ' ὅσοι μετόπισθε ποτὶ ζόφον ἡερόεντα. ν. 240,

is of course without bearing on the subject. The other

ὦ φίλοι, οὐ γάρ τ' ἴδμεν ὅπη ζόφος οὐδ' ὅπη ἡώς,
 οὐδ' ὅπη ἡέλιος φασείμβροτος εἶς' ὑπὸ γαίαν
 οὐδ' ὅπη ἀννεῖται. κ. 190,

is inconsistent according to the ordinary interpretation, which, taking for granted that ζόφος and ἡώς always have their later meaning of W. and E., makes the following clauses mere repetition. It is possible, though by no means necessary², to regard these as four distinct alternatives.

In a former paper (*supr.* p. 50) I derived ἡώς from the root *ves* 'shine' with the intensive prefix *ē*. The side-light thus thrown on its primitive meaning agrees with its use in all these passages, and affords an exact parallel to ζόφος. Their developement may be traced through the following stages. ἡώς is first of all 'sunshine,' 'daylight,' 'the realms of day,' whence its close conjunction with ἡέλιος, just as ζόφος is 'darkness,' 'the realms of night,' and is coupled with Ἑρεβος. Then, in two passages certainly, πρὸς ζόφον and πρὸς ἡῶ τ' ἡέλιόν τε give generally N. and S. direction. Next by a very simple metonymy they come to signify the 'gates of light' and 'of darkness' respectively, and so 'sunrise' and 'sunset.'

¹ I cannot meet the argument that Homer meant E. and W. in each case because he did not know what he was

talking about. It is unanswerable.

² The passage bears signs of late origin in the elision before (f)ἴδμεν.

There is accordingly no reason for separating M. 239 sq from ι. 25 sq. and μ. 80 sq., especially when by so doing we introduce a custom in augury not found elsewhere. Accordingly here also the observer faces the E. and the flight ἐπιδέξια is towards the S., and we may suppose that the primitive methods of divination were the same for Greeks and for Romans, and in all probability drawn by both alike from Oriental sources¹.

The direction ἐπὶ δεξιά being that of the apparent course of the sun is another reason for its auspicious character. This indeed is not confined to the Greeks and Romans. In Jamieson's *Dictionary s. v. Widdersinnis* there is an interesting quotation to shew the Gaelic observance of it, and he adds: "the custom of sending drink round a company from left to right is by many supposed to be a vestige of the same superstition. There are still some, even in the Lowlands, who would reckon it unlucky to take the opposite course"².

¹ Whether this explanation of πρὸς ζόφον and πρὸς ἡῶ κτλ. be accepted or not, these lines form no justification for the statement which is apparently built on them and them alone, that the Greek augurs looked to the N. I regard the statement on Hector's part as quite general, but if in spite of ι. 26 and μ. 81 πρὸς ζόφον cannot mean "towards the N.," his statement must be regarded as referring to this particular omen, to which it would then apply, as he and Polydamas are facing towards the Greek camp and consequently N. This is the view taken by [Achilles Tatius] in a passage which might have been quoted above if more examples had been necessary:—τὰ μὲν οὖν τῆς θέσεως ταύτ' ἐστίν· τὸν δὲ ἐξηγούμενον ἐν δεξιᾷ χρὴ τὸν βόρειον πόλον ἔχειν καὶ ἐν ἀριστερᾷ τὸν νότιον. ἐπειδὴ αἱ ἄρκτοι ἐπὶ ἀνατολῶν ἐν δεξιᾷ κεῖνται, ἐν ἀριστερᾷ δὲ ὁ νότος. τινὲς δὲ τῶν ἐξηγουμένων βούλονται ἐμπροσθεν μὲν τὰς ἄρκτους ὀπίσω δὲ τὸν νότον, δεξιά δὲ

τὰς ἀνατολὰς ἀριστερὰ δὲ τὴν δύσιν ἔχειν, ὡς ἀπὸ τῶν Ὀμηρικῶν ἐπὶ κινήθεντες Εἰτ' ἐπὶ δεξι' ἴωσι κτλ., οὐκ εἰδότες ὡς ὁ ποιητὴς πρὸς τὴν τῶν τόπων ἐκείνων θέσιν, δεξιά μὲν τὰ ἀνατολικά, ἀριστερὰ δὲ τὰ δυτικά εἶπεν. ἡ δὲ τοῦ κόσμου θέσις κατὰ πρόσωπον δεξιὰς ἔχει τὰς ἄρκτους, καὶ οὕτως τιθέναι χρὴ τὴν σφαῖραν τὸν ἐξηγούμενον. *Isag. in Phaenom.* 35, p. 161 B. This emphatic statement combined with the silence of Plutarch (*Quaest. Rom.* l. c.) might have prevented any false deductions from the Homeric passage.

² The question then naturally arises: why has modern custom changed this and made the reverse order universal? The answer is that the wide-spread use of time-pieces makes us prefer 'clockwise' to 'counter-clockwise.' But if counter-clockwise was the favoured direction previously, why were the first clocks not made with their figures reversed? It has been asserted that this was because the upper half

In applying ἐπὶ δεξιά, the lucky direction, to the uses of daily life, one became so especially frequent as to develop a semi-technical acceptation¹. This is its application to the course of the wine at feasts, sacrifices, and the like. Homer illustrates this in four passages:

αὐτὰρ ὁ τοῖς ἄλλοισι θεοῖς ἐνδέξια πᾶσιν
οἶνοχόει... A. 597.

κῆρυξ δὲ φέρων ἀν' ὄμιλον ἀπάντη
δεῖξ' ἐνδέξια πᾶσιν ἀριστήεσσιν Ἀχαιῶν. H. 183.

βῆ δ' ἵμεν αἰτήσων ἐνδέξια φῶτα ἕκαστον. ρ. 365.

ὄρνησθ' ἐξείης ἐπιδέξια πάντες ἐταῖροι
ἀρξάμενοι τοῦ χώρου ὅθεν τέ περ οἶνοχοεύει. φ. 141.

Some of the older commentators wished to give to the adverb here its later meaning of 'dexterously,' but this is much less appropriate, especially in the last passage (*v. Buttm. Lexil.* p. 173). It must be noted that in all the passages the wine circulates from left to right of the feasters, &c. This of course is not altered by the presence of a wine-server, whose duty accordingly begins on his extreme right and proceeds leftwards. This use has become so specialised that the inconsistency of asserting that the cupbearer goes ἐπιδέξια in such cases is not perceived.

In this connection may be mentioned an important passage in Herodotus which has occasioned some difficulty. He is enumerating the various points in which the Egyptian usage is contrary to Greek, and concludes γράμματα γράφουσι καὶ

was made to correspond to the direction of our writing. This is not probable. Rather the first clock face was modelled on a sun-dial and consequently the hands follow the direction of the sun's shadow.

¹ There appears to be no fixed rule when ἐπὶ δεξιά and when ἐπιδέξια should be written. It is best to write it as two words, whenever it can be construed as a syntactic expression,

and to reserve ἐπιδέξια for those instances in which the development of a secondary sense proves that it is a true adverb. The only passage in Homer where it is strictly literal is H. 238. ἐπὶ δεξιά is also correct in passages like Hd. II. 93, Ar. Av. 1493, but probably not in Pind. P. vi. 19, [Theoc.] xxv. 18, where the addition of χειρὸς shews the necessity of marking the local sense was felt.

λογίζονται ψήφοισι, "Ἕλληνες μὲν ἀπὸ τῶν ἀριστερῶν ἐπὶ τὰ δεξιὰ φέροντες τὴν χεῖρα, Αἰγύπτιοι δὲ ἀπὸ τῶν δεξιῶν ἐπὶ τὰ ἀριστερά· καὶ ταῦτα ποιεῦντες αὐτοὶ μὲν φασὶ ἐπιδέξια ποιέειν, "Ἕλληνας δὲ ἐπ' ἀριστερά. Π. 36.

The following quotations illustrate the views which have been taken of this passage :

Littlebury translates "but the Ægyptians...from the Right to the Left. And yet pretend in doing so, that their line tends to the Right and ours to the Left"¹.

Mr G. C. Macaulay in his translation (Vol. I. p. 132) renders the last words "and doing so they say that they do it themselves rightwise and the Hellenes leftwise."

Brugsch, *Gram. Démotique* (p. 15, § 27), says: "Déjà plus haut j'ai fait la remarque que le démotique s'écrit de droit à gauche. Sous ce titre Hérodote nous a conservé une notice infiniment curieuse qui lui est venue des Égyptiens et que voici: γράμματα κτλ. D'après notre connaissance du démotique, il est évident que cette remarque ne concerne point la direction générale de l'écriture déjà tracée mais uniquement la manière dont on devait écrire les signes, c'est-à-dire en les commençant du côté gauche et en les terminant à droite etc."

The first of these three typical views regards the assertion of the Egyptians as a pure absurdity, which is about as probable as it is that a left-handed man should accuse the normal practice of being *gauche*. The second view, which Mr Macaulay follows if I understand his words rightly, gives to ἐπιδέξια the sense of 'skilfully,' "they claim that they do it dexterously and the Greeks the reverse." Here also the retort is too obvious "the mode which proceeds ἐπὶ δεξιὰ, must be the ἐπιδέξια mode." This view seems to have found most favour and is certainly the best of the three, but it involves a sense of ἐπιδέξια which first occurs in much later authors and hence can only be accepted in default of a better. The third explanation is undoubtedly ingenious but will not bear scrutiny. If stated in full it involves the following interpretation: the Greeks write from left to right and the Egyptians from right to left,

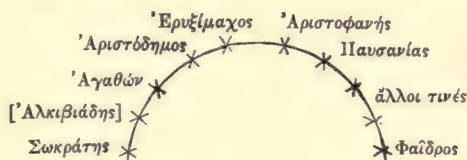
¹ So Voss l.c. 43.

but, as each nation forms its characters in the opposite direction to that of the writing, the Egyptians claim to write ἐπιδέξια. The proof of the statement concerning the Egyptian characters rests on some indications on papyri, which do not seem very cogent. On the other hand in the hieroglyphic alphabet, on which the demotic is founded, many characters represented animals (*v.* Rawlinson II. p. 261) and, although hieroglyphs are written in either direction, the head of the animal is always turned towards the direction from which the hand comes. Thus (unless the writer always began at the tail) the character must have been formed in the same direction as the writing, in the hieroglyphic alphabet, and the same would naturally hold of its derivatives, the hieratic and demotic. Of course the assertion for Greek is supported by no particle of evidence. If any more evidence is needed to disprove this view it can be found in the language of Herodotus, who has carefully left out the important fact of the story, although it would have given him another instance for his topic, which is the perverse customs of the Egyptians—and has moreover selected the unfortunately ambiguous words γράμματα γράφουσι.

The passage at once becomes clear if we bear in mind the specialised use of ἐπιδέξια in Homer, which has just been referred to. The four passages quoted above shew that, when a single individual was referred to in relation to a line of others, he was said to go ἐπιδέξια if he began at his extreme right, because the adverb had reference to the right and left of those who formed the line or circle. It is to this that the Egyptian statement alludes, comparing the written characters to a row of feasters and the writer to the cupbearer. Just as in A. 597 Hephaestus goes ἐπιδέξια beginning at the extreme right (to him) of the row of gods, and as in H. 184 the herald goes ἐπιδέξια beginning at the extreme right (to him) of a row of chiefs, so an Egyptian goes ἐπιδέξια beginning at the extreme right (to him) of a row of letters.

Perhaps an illustration may make this clearer. In Plato *Symp.* 177 D we have δοκεῖ γάρ μοι χρῆναι ἕκαστον ἡμῶν λόγον εἰπεῖν ἔπαινον Ἐρωτος ἐπιδέξια (*vulg.* ἐπὶ δεξιὰ) ὡς ἂν δύνηται κάλλιστον, ἀρχεῖν δὲ Φαῖδρον πρῶτον, ἐπειδὴ καὶ

πρῶτος κατακεῖται καὶ ἔστιν ἅμα πατήρ τοῦ λόγου. As may easily be seen from the context the following diagram shews the position of the guests.



Now if a Greek were called upon to write down the names in the above diagram he would start with Σωκράτης and end with Φαῖδρος, but, as the text proves, the ἐπιδέξια order begins with Phaedrus and ends with Socrates, so that an Egyptian who would begin with ΟΟΓΔΙΑΦ on the extreme right might justly claim to be writing ἐπιδέξια.

It may at first sight seem absurd to suppose that written characters could be so far personified as to be credited with a subjective right and left. The thought however is the same as in Aristotle *Metaph.* N. 6, p. 1093^a 30. He there asserts that the Homeric line (viz. the purely dactylic) βαίνεται ἐν μὲν τῷ δεξιῷ ἐννέα συλλαβαῖς, ἐν δὲ τῷ ἀριστερῷ ὀκτώ where the *right-hand* side of the line is clearly the *first* half, i.e. right and left are applied to the line subjectively¹, and, as the scholiast says, τὸ δεξιόν is τὸ ἡμισυ τὸ ἀπὸ τῆς ἀρχῆς ἕως τοῦ μέσου—the first three feet being dactyls contain nine syllables, the last three, two dactyls and a spondee, eight. Its perfectly matter-of-course use in this place by Aristotle (as it were, the *heraldic*

¹ This explanation and the quotation from the scholiast which follows I owe to Dr Henry Jackson's lectures, and consequently I am not compelled to respect, as I otherwise should, the authority of Bonitz, who takes the contrary view. Against his explanation may be urged, in addition to the authority of the scholiast who makes his statement most emphatically twice over, that it is not likely A. would have mentioned the second half of the line first, especially when it contains

the larger number. Besides, his division of the line at the caesura limits the statement to dactylic lines with feminine caesura, of which there is no hint in the text; that is to say, his explanation suits

ἀλλ' ἔθι μή μ' ἐρέθιζε || σωώτερος ὥς κε νέηαι

but not

τοῖσι δ' ἀνιστάμενος || μετέφη πόδας ὠκὺς Ἀχιλλεύς.

right and left) is a strong argument for a similar explanation of the passage in Herodotus¹.

To return to ἐπιδέξια—the sense at present under discussion is frequent throughout Greek literature. In Plato besides the passage already quoted it occurs *Symp.* 214 B, and in *Rep.* IV. 420 E, where it seems to make better sense than “*scite, eleganter*” as some translate it. It bears the same sense in the very obscure quotation from Anaxandrides ap. *Athen.* XI. 10, p. 463 E sq. and in a quotation from Hippias, *Ib.* XII. 74, p. 600 E.

Another specialised sense of ἐπιδέξια is in connection with the mode of wearing the ἱμάτιον. Thus Aristophanes,

οὗτος τί δρᾷς; ἐπ’ ἀριστέρ’ οὕτως ἀμπέχει;
οὐ μεταβαλεῖ θοίμάτιον ὧδ’ ἐπὶ δεξιᾷ; (l. ἐπιδέξια)
Av. 1567.

Here the himation is to be thrown over the *left* shoulder: consequently the adverb does not refer to the motion implied in μεταβαλεῖ but to the direction in which the folds run when the garment is adjusted, viz. from left to right. So also Plato *Theaetetus*, p. 175 D f.

P. 175 D f. Οὗτος δὴ ἐκατέρου τρόπος, ὃ Θεόδωρε, ὁ μὲν τῷ ὄντι ἐν ἐλευθερίᾳ τε καὶ σχολῇ τεθραμμένον, ὃν δὴ φιλόσοφον καλεῖς, ὃ ἀνεμέσητον εὐήθει δοκεῖν καὶ οὐδενὶ εἶναι, ὅταν εἰς δουλικά ἐμπέσῃ διακονήματα, οἷον στρωματόδεσμον μὴ ἐπισταμένον συσκευάσασθαι μηδὲ ὄψον ἡδύναι ἢ θώπας λόγους· ὁ δ’ αὖ τὰ μὲν τοιαῦτα πάντα δυναμένου τορῶς τε καὶ ὀξέως διακοπεῖν, ἀναβάλλεσθαι δὲ οὐκ ἐπισταμένου ἐπιδέξια ἐλευθέρως οὐδέ γ’ ἀρμονίαν λόγων λαβόντος ὀρθῶς ὑμνησαι θεῶν τε καὶ ἀνδρῶν εὐδαιμόνων βίον ἀληθῆ.

Here the double ambiguity in ἐπιδέξια and ἀναβάλλεσθαι caused the interpretation to be doubted at a very early date. Thus one of the scholia runs τῶν γὰρ ἀπαιδευτῶν καὶ τὸ ἐνδυμα καὶ τὸ σχῆμα θορυβώδες. ἄλλοι δὲ τὸ ἀναβάλλεσθαι ἐπὶ τῶν

¹ The Greeks seem on the whole to have preferred the subjective use of δεξιός whenever the circumstances

made it possible. Thus in Aristotle *Probl.* κς’ 31 p. 943^b28, ζέφυρος is said to be ἐπὶ δεξιᾷ of βορέας.

κιθαρῶν ἀκούουσι τῆς ᾠδῆς, οἷον τὸ ἄρχεσθαι κιθαρίζειν. καὶ οἶμαι οὐ κακῶς· τό τε γὰρ πρόκρουμα τῆς κιθάρας ἀναβολὴ καλεῖται.

Athenaeus quotes the passage (I. 18, p. 21 B) and interprets it as referring to the himation. On the other hand Themistius is clearly alluding to it when he says (*Or.* XXI. p. 263 D)

ἄρα ὑμῖν ὁ τοιοῦτος ὕπαρ τε καὶ ὄναρ δοκεῖ ἂν ποτε δυνηθῆναι ἀφέμενος τοῦ σκαιοῦ τε καὶ ἐπαρισστεροῦ τρόπου ἐπὶ δεξιᾷ ἀναβάλλεσθαι καὶ λαβὼν ἁρμονίαν λόγων ἀληθινὴν ὑμνῆσαι βίον θεῶν τε καὶ ἀνθρώπων.

[*Puto legendum ἐπιδέξια ἀναβάλλεσθαι, hoc est, scite atque eleganter hymnum auspicari. Sic enim ἀναβάλλεσθαι sumitur. Harduin.*]

And this explanation of Harduin's is right, for the omission of ἐλευθέρως makes it impossible that Themistius could have intended the other. This view seems to have been taken by editors of Plato down to the time of Casaubon, to judge from the words of his note on Athenaeus l. c. (*Notes*, p. 53) *Platonis interpretes viri eruditissimi vocem ἀναβάλλεσθαι aliter exceperunt sed potior apud me Athenaei sententiam qui ad amictum refert. Casaubon's authority however seems to have been followed by all subsequent editors, with an occasional protest (e.g. Sallier *Mém. Litt. Acad. Inscr.* (1734-7) Vol. XIII. pp. 323 and 330). The case could not be better put than it is in Stallbaum's note, and he decides for Casaubon's view, which is clearly the better of the two. A writer of Plato's stamp however does not use his expressions haphazard so to speak, and I do not believe he would use words so completely ambiguous unintentionally. On the other hand none of his readers can have failed to notice the delightful way in which he uses a technical expression with a side-glance at its literal sense, and *vice-versa*, or at other times plays with the different meanings of a single word. So also here, if we suppose him, as I believe we must, to have selected the expression deliberately and to intend an allusion to both senses, we obtain a fuller and closer correspondence to the first half of the sentence. A servile nature can 'pack a bag' (mere knack of neatness), 'season*

dishes or cook' (an accomplishment, but still superficial), 'be a master in the use of words' (the deepest culture of which it is capable). So also a free nature can 'wear his cloak with an air' (mere elegant knack), 'play featly on the lyre in company'¹ (an accomplishment, but comparatively superficial), 'hymn the life of Truth' (the truest and deepest culture).

Up to this point we have been dealing entirely with ἐπιδέξια (ἐνδ.) as an advb. which originated in the syntactical collocation of the words ἐπὶ or ἐν δεξιά, which is of course a common mode of forming adverbs in other languages also. They are not therefore to be regarded as the neuter plurals of the adjectives ἐπιδέξιος and ἐνδέξιος respectively. The use of ἐπιδέξιος seems to be first found in Aeschines I. (c. *Timarch.*) § 178, who also uses the regularly formed advb. ἐπιδεξίως II. (*de F. L.*) § 124. It is not uncommon in later writers: Aristotle uses it three times, Theocritus once (*Epigr.* XIX. 5), and it is frequent in Polybius and Dio Cassius (v. the quotations from Liddell and Scott *supr.*).

The natural explanation at first sight is that ἐπιδέξιος has been formed from ἐπιδέξια, which was regarded as an adverbial neut. pl., just as in Latin *sē(d) dolō* became *sēdulō* and this induced an adjective *sēdulus* (cf. Stolz *Handb.* II.² p. 324). The difficulty is that ἐπιδέξιος always means "clever, dexterous," while ἐπιδέξια, as we have seen, is either literal or used in one of two special senses. It is true that ἀναβάλλεσθαι ἐπιδέξια might come to mean "wearing one's cloak elegantly" and so "cleverly," but this use is scarcely important enough to lead to such a large new development. A simpler explanation of ἐπιδέξιος is that given by Cope (*Arist. Rhet.* Vol. II. p. 48 f.), who makes it a compound of δεξιὸς with the intensive prefix ἐπί, as ἐπιλήσμων is of λήσμων. In this way the meaning "skilful, dexterous," is reached much more easily.

The objection will at once be raised that the preposition ἐν is not thus used to limit the meaning of an adjective, and con-

¹ The examples we have already had sufficiently dispose of Prof. Campbell's objection that ἐπιδέξια could not be applied to a single person of the

company: it has been shewn even to apply to a single person outside the company.

sequently ἐνδέξιος must be formed from ἐνδέξια, whence it naturally follows that ἐπιδέξιος is a similar formation. This reasoning is good but involves an examination of the authorities for ἐνδέξιος. In I 236,

Ζεὺς δέ σφι Κρονίδης ἐνδέξια σήματα φαίνων
ἀστράπτει,

ἐνδέξια is not an adj. agreeing with σήματα but an advb. qualifying σήματα φαίνων ἀστράπτει, cf. B 353. The other three passages in Homer, A 597, H 184, ρ 365, have already been cited, and in all it is the adverb. So also in the later Epic, as Callim. *Hym. Jon.* 69,

σὼν τεράων, ἅτ' ἐμοῖσι φίλοις ἐνδέξια φαίνοις.

The passage quoted by Liddell and Scott from Euripides *Hipp.* 1360 is doubtful. Editors are divided between

τίς ἐφέστηκε ἐνδέξια πλευροῖς¹,

as L. and S. apparently read², and

τίς ἐφέστηκεν δεξιὰ πλευροῖς³.

The objection to the first is that it violates the anapaestic pause, to the second that δεξιὰ as an advb. is a ἅπαξ λεγόμενον in classical authors: it occurs however in the passage quoted above from [Achill. Tat.]. To my mind Hermann's

τίς ἐφέστηκεν δεξιόπλευρος

is convincing.

The passage from the *Cyclops* (l. 6), if any reliance is to be placed on it, stands quite apart, and appears to be jestingly formed from ἐν δεξιᾷ. In any case it affords no parallel to ἐπιδέξιος.

The only passage in which ἐνδέξιος is used in the sense of 'skilful' 'clever' is in *Hymn. Hom. Merc.*,

καὶ γὰρ ἐγὼ Μούσῃσιν Ὀλυμπιάδεσσιν ὀπηδός,
τῇσι χοροὶ τε μέλουσι καὶ ἀγλαὸς ὕμνος ἀοιδῆς

¹ e.g. Musgrave.

² Is it possible that they regard ἐνδέξια here as a feminine? If not,

what authority have they for making ἐνδέξιος an adj. of three terminations?

³ e.g. Kirchhoff.

καὶ μολπὴ τεθαλυῖα καὶ ἰμερόεις βρόμος αὐλῶν
 ἄλλ' οὐπω τί μοι ὦδε μετὰ φρεσὶν ἄλλο μέλησεν,
 οἷα νέων¹ θαλίσς ἐνδέξια ἔργα πέλονται. l. 450.

The last line is clearly an interpolation: the sense is complete without it; it belongs to an epoch when *οἶος* and *ὅσος* had become interchangeable; it is a clumsy attempt to sum up the enumerations in the preceding lines (a class to which belongs a considerable number of interpolations); and as an expansion of *ἄλλο* it yields no logical construction. When we add to these facts that it has required emendation to make sense at all, and that it contains the *only* example of this sense of *ἐνδέξια*, we may think ourselves justified in concluding that the line is of considerably later date than even this latest of the Hymns and that the interpolator, knowing that Homer used both *ἐπιδέξια* and *ἐνδέξια*, and knowing that *ἐπιδέξιος* could mean 'clever,' jumped at the conclusion that *ἐνδέξιος* also existed and could mean 'clever.'

From this brief notice of the principal passages which throw light on the subject, a scheme may be constructed in which chronological order and order of development coincide.

I. Adverbial.

A. ἐπὶ δεξιά

- a. "Towards the right." H 238, Ar. *Pac.* 957, Arist. *Probl.* κς' 12, p. 941^b 11, *Ib.* 31. 943^b 28. In Hdt. II. 36 ἐπὶ τὰ δεξιά is used instead to distinguish this sense from B b.
- b. "On the right." Hdt. I. 51, IV. 191, VI. 53; Plato *Parm.* 129 c; Xen. *Anab.* VI. 2. 1. Defined by χειρός Pind. *Pyth.* VI. 19, [Theocr.] xxv. 18. Possibly should be written as one word, but is a distinct line of development from B.

Hence τὰ ἐπὶ δεξιά "the right side" Hdt. II. 93. Ar. *Av.* 1493.

¹ θεωω corr. Gemoll.

B. ἐπιδέξια

- a. Of an omen "left to right" and hence "lucky." B 353, M 239.
- b. Of the wine-cup "left to right" (and hence "lucky") whence "counter-clockwise" generally. ϕ 141, Hdt. II. 36, Plato *Theaet.* 175 E, *Symp.* 177 D, 214 B, *Rep.* IV. 420 E, Athenaeus &c.
- c. Of the himation "left to right" and hence "gracefully," "in gentlemanly fashion." Ar. *Av.* 1568, Plato *Theaet.* 175 E, Athen. I. 18, p. 21 B.

C. ἐνδέξια

- a. Of an omen, like B a. I 236.
- b. Of the wine-cup, like B b. A 597, H 184, ρ 365.

II. Adjectival.

- A. ἐπιδέξιος "clever," "dexterous" (= δεξιός with ἐπι prefixed). Aeschines I. (c. *Timarch.*) § 178; Arist. *Eth. Nic.* IV. 14. 1128^a 17, IX. 11. 1171^b 3; *Rhet.* II. 4. 1381^a 34; Theocr. *Epigr.* XIX. 5; Polyb. v. 39. 6; Dio Cass. LXIX. 10 etc.

Of this the proper adverb is ἐπιδεξίως, as Aesch. II. (*F. L.*) § 124; Polyb. III. 19. 13 etc.

B. ἐνδέξιος

- a. = ἐν δεξιᾷ, Eurip. *Cycl.* 6.
- b. "clever," formed on the false proportion ἐπιδέξια : ἐπιδέξιος = ἐνδέξια : ἐνδέξιος, *Hymn. Hom. Merc.* 454.

(6) ON THE INDO-EUROPEAN WORDS FOR
FOX AND WOLF.

(Read before the Cambridge Philological Society on Feb. 25, 1892,
and printed in *Camb. Philol. Transactions*, Vol. III. p. 187.)

FOX AND WOLF.

"There's always an amicable way out of a dissension, if we get rid of *Lupus and Vulpus*." *One of Our Conquerors*, p. 57.

THE exact position which we are to ascribe to the home of the Indo-European language is perhaps still unsettled, but a recent paper by Dr Hirt in *Indog. Forsch.* I. p. 464 ff. has at least shewn a high probability for the neighbourhood of the Baltic. In this case both fox and wolf must have been familiar incidents in the experience of the inhabitants, and it is natural to suppose that they were not nameless. In fact most inquirers have assumed that the various words in the separate languages are traceable back to I.-E. forms, and that the two animals in question were not only known but also distinguished in the primeval epoch. The latter assumption is I think unjustifiable, and, as far as can be gathered from the scanty indications which our present knowledge affords, both psychological and philological probabilities are against it.

As regards the former, it is of course difficult to speak with any confidence, for the nearest parallel case which we can observe—the growth of language in a child—is not necessarily analogous in all respects to the primitive development. This much however can be affirmed¹. Language implies thought, and thought implies the existence of mental systems. Such mental systems would at first have a purely subjective character; the members of the system would agree in their relation

¹ For much of what follows refer to Mr G. F. Stout's papers on *Apperception and the Movement of Attention*, and *Thought and Language*, in *Mind*, Vol. xvi. pp. 23 and 181.

towards the sentient subject, *and not necessarily otherwise.* (Period I.)

Subsequently, as mind developed (with the aid of language), a new and more advanced set of mental systems would be formed, of which the members would have a constant objective relation to each other. (Period II.)

Thus for example Steinthal relates (*Einkl.* p. 403) that his son having acquired the word "nähen" used it not only for "sewing," but also for "mending a broken toy," and even to express a pair of scissors. It is probable that the primitive development was similar, that originally a certain combination of sounds expressed all that affected the speaker in a certain way, so that the same natural object, e.g. a certain tree, would be 'named' differently according as the idea of eating its fruit or of using its timber was uppermost, while on the other hand different natural objects which agreed in the prominent characteristic would not be distinguished in speech. In this second period a notable advance has been made; the distinction existing in nature between different classes of objects has impressed itself on the mind, and consequently class-names are associated with them, which for the most part come from the earlier descriptive epithets.

Moreover from the facts to be observed in the Indo-European languages it would *not* seem that the first period is to be relegated to an inconceivable antiquity. On the contrary these facts appear to shew that, if the transition process had begun during the period of unity, it had not been completed when the dispersal began.

The following are a few of the more striking or less familiar of these facts; others can easily be added from Dr Hirt's paper above referred to, or from Fick's *Vergl. Wörterbuch* ed. 4.

Gk. *φηγός*, Lat. *fāgus*, Eng. *beech*. I.-E. *bhāḡos* connected with *bhaḡ* "eat" and therefore applied to any tree producing edible fruit.

Skt. *bhūrja-*, Lit. *beržas*, Eng. *birch*. I.-E. *bherḡos* connected with *bherḡ*, "be white," Lit. *berszti*, and so "the white tree."

Gk. *ἐλίκη* (Arcad.), Lat. *salix*, O. H. G. *salaha* probably connected with *sel* "to move," and so "the pliant."

Gk. *μᾶλον*, Lat. *mālum*, I.-E. *mālom* from $\sqrt{mā}$ (cf. *mā-ter-*) and so any juicy fruit.

Skt. *avi*, Gk. *ὄφις*, *οἶς*, Lat. (Sab. ?) *ovis*, Lit. *avis*, and Gk. *ὄφι-* in **ὄφιωνός*, *οἰωνός*, Lat. *avis*, both connected with I.-E. *ovis* and the root *eu* "to draw, pull, or pluck" (*Grds.* I. § 171-2, and *Cl. Rev.* IV. 273 b [= p. 63 sup., where cf. footnote 4]), applied equally to the animal from which wool, and to the bird from which feathers were plucked.

It is clear then that, if the names applied to fox and wolf can be shewn to have had a similar subjective sense, the existence of two such "descriptive" names will not prove that the two animals were objectively distinguished.

The object of the present paper is to shew that the more important names for these animals in the several I.-E. languages can ultimately be traced back to derivatives of two roots, the one being *uel* (cf. Lat. *vel-lo*, *vol-nus*), the other *lup* (cf. Skt. *rup*, *lup*¹), each having alike the sense of "plunder" or "destroy." These two roots were modified by formative suffixes and the scheme of their development appears to have been

Root	F. S.	Indo-European	Facts
<i>leup</i> <i>loup</i> <i>lō(u)p</i> <i>lup</i>	-ēko- -ó-	<i>leupēko-</i> } <i>loupēko-</i> } <i>lōpēko-</i> <i>lupó-</i>	<i>lopāza-</i> <i>αλοιwēs</i> <i>ἀλώπηξ</i> <i>lupus</i>
<i>uel</i> (<i>uol</i>) <i>ul</i> <i>ul</i>	-qó- -iό-	<i>ulqó-</i> <i>uljiό-</i>	<i>vḡká-</i> <i>vīlka-</i> (<i>f</i>) <i>λύκος</i> <i>vulpēs</i> <i>vulfs</i> <i>gail</i>

¹ In later Skt. both these forms of root are found: in the *Rig-Veda* only *ārupita-* (Pada *ar-*). That an I.-E. $\sqrt{rup}$, *rup*, also existed is proved by Latin *rumpo*. For *leup*, *lup*, may be

compared Albanian *tup*, 'devour,' in which *t* = I.-E. *l* as in *tere* = *ώλενη* (G. Meyer, *Etym. Wört. d. Alb. Spr.* p. 233). [Cf. pp. 233 and 237 inf. C.]

As preliminary remarks on the above table I would observe that the form *lou̥pēko-* is slightly more probable than *leupēko-* as the original of *lopā̄ḡa-* and *alowēs* (either being phonologically accurate) because it is not certain that I.-E. admitted two e-vowels in consecutive syllables; and as regards the form *ulqó-*, that we have the same root and suffix combined with different accentual conditions in *uelq-* (Lit. *velkù* etc.¹), *Cl. Rev.* vi. 58 [= p. 126 inf.]. Further it will be noticed that I have identified words which do not correspond in sense, by which I intend to imply that the names had not lost their 'descriptive' sense before the separation of the languages.

I shall now proceed to discuss separately the modes in which these words are presented in the several languages.

I. Sanskrit.

As *lopā̄ḡa-* and *vṛká-* are the direct equivalents of *lou̥pēko-* and *ulqó-*, these two words call for no further comment.

The form *lopā̄ka-* has been supposed to be borrowed from *ἀλώπηξ* (cf. *Idg. Forsch.* i. 328 n²), but it is more probably a Sanskrit diminutive.

II. Armenian.

1. *alowēs*.

This form regularly represents I.-E. *lou̥pēk-*, and so can also be referred to *lou̥pēko-* by a transition to the consonantal flexion which may further be assumed in Greek (cf. *Brgm. Grds.* ii. p. 237). This is rendered almost certain by the occurrence of a gen. sg. *alovesoy*, although the more common gen. sg. *alovesow* and gen. pl. *alovesowç* point to a *u*-stem (which is remarkable). The form *alvēs* (in *alvēsowt'i* 'calliditas,' also independently) does not represent an I.-E. ablaut-form **lupēk-* but is due to Armenian changes².

¹ The meaning 'plunder' for this combination of root and suffix is best given by Gothic *wilwan*, which probably goes back to I.-E. *uelq-*. De

Saussure, *Mém. Soc. Ling.* vi. 338.

² Compare generally Bartholomae *B. B.* x. 294; Hübschmann, *Arm. Stud.* i. no. 62.

2. *gail*.

This form is of especial importance for my analysis. It is usually referred to $\gamma\iota\lambda\acute{o}$ -, but Hübschmann with his ordinary logical exactness adds "Für indogerm. **vlko-* würde ich im Armenischen **galg* erwarten. *gail* entstand aus **galy* wie *ail* = *alius* aus **aly* entstanden ist. Wie aber ging **galy* aus *galg* hervor? ¹"

Accordingly on p. 74 he gives this as an isolated instance of Arm. *y* = I.-E. *k* [q]. This exception is improbable and unnecessary. Armenian *y* regularly represents I.-E. *j* medial; *yauray* = *πάτριος* and *yisun* from *hing* are to be classed with *y-arnem* = *ὄρνυμαι* etc. in which *y* is prothetic: that is, the initial *p* has been lost, through *h*, and then the *y* has come to precede the initial vowel. This prothetic *y* is probably due to wrong division, possibly from the final *y* of certain genitives and 3d persons singular, but it may also have a different explanation. It is at all events incorrect to say that *y* in these words represents *p*, or to say that it represents *t* in the 3d sg., as *alay*, *berē*; rather, I.-E. *t* is lost between vowels, probably through a spirant, so that *bhereti* became *bhereθi*, *berei*, *berey*, *berē*, successively. Accordingly all shadow of justification for Arm. *y* = I.-E. *q* disappears, and it is absolutely necessary to refer *gail* to $\gamma\iota\lambda\acute{o}$ - as *ail* to $\iota\lambda\acute{o}$ - (which gives *ἄλλος* as $\iota\lambda\acute{o}$ - does *alius*). If then the two forms $\gamma\iota\lambda\acute{o}$ - and $\gamma\iota\lambda\acute{o}$ - are proved for I.-E., it is clear that we are dealing with the two very common formative suffixes -*go*- and -*io*-, and therefore the analysis— $\gamma\iota\lambda\acute{o}$ —, which has often been suggested, becomes convincing.

III. Greek.

1. *ἀλώπηξ*.

As explained above, it is to be supposed that this word has changed from the -*o*- to the consonantal flexion. Its form however has occasioned some difficulty, and Bartholomae (*B. B.* x. 294) even regards it as a borrowed word from some Oriental source². This is of course, for the name of so common an

¹ *Arm. Stud.* i. p. 24. Cf. Barthol. *B. B.* xvii. 94 note.

² So more recently G. Meyer in *Idg.*

Forsch. i. p. 328 n., who seems to labour under some strange misapprehension. Armenian *αλωε̃ς* can not

animal, improbable, and, as the word appears to occur as early as Solon, it becomes still more unlikely. Admitting that *lōyp* became *lōp*, the equation of *ἀλώπηξ* and *lopāca-* is regular. It is true that Brugmann, *Grundriss* I. § 188, questions the possibility of this loss of *u* after *ō* except before *m* under certain conditions. That this view is too narrow has been lately proved by Bechtel (*Hauptprob.* p. 273 ff.) who extends the cases to *m*, *l*, *r*, *s*, *t* and sees no reason to limit them to any particular consonants. As ample material may be found by reference to him, I need only add one or two additional examples which suggest themselves, such as Feist's equation¹ of O. Norse *ból* with *bú* (adding *φωλεός* and Skt. *bhūmi-* with hesitation); so Greek *κωκύω* goes with Skt. *kócati*, Lith. *kaukti*; *κρώζω* goes with *κρανγή* and both with Skt. *kruç*; *ὠχρός* again most probably is connected with *αὐχμέω*, of which the original sense must have been "to be yellow," comparing the sense of *αὐχμηρός* and *αὐχμῆς τε κακῶς καὶ ἀεικέα ἔσσαι* (*ω* 250). That the Latins should have called gold *aŭh-ro-m* "the yellow (metal)" rather than *aŭs-o-m* "the (metal) of the dawn" seems to me a natural supposition², but if Lith. *auksas* was really borrowed from Latin it appears to confirm Festus' testimony to *ausom*. On the other hand *aurum* appears as an exception in Mr Conway's *Verner's Law in Italy* (p. 79), and he has to explain it by popular connexion with *Aurora*.

The case for *lōyp* becoming *lōp* for the European group of languages at all events is therefore fully established, and although the prothetic vowel in Greek needs discussion, it must be reserved for a full examination of that phenomenon.

2. λύκος.

The immediate precursor of this Greek form must have been *lukos*, and not *uľqos* (which would give **ἀλπος*) or even *uľuqos*, as there is no trace of initial *ř*³. Of such a form as

represent *alopēs* except as = [a] *lopēkos* or [a] *lopēsos*; of these the former corresponds exactly to *lopāca-*, the latter to nothing whatsoever. Yet *ἀλώπηξ* cannot possibly = *lopāca-*, but is clearly identical with *alouēs*!!

¹ *Got. Etym.* p. 16.

² It is probable that *h* from *ǵh* would vanish before *r*, although instances of it do not seem to occur.

³ e.g. οἱ δὲ λύκοι ὦς ends the line π 156.

lukos the only explanation rests upon a theory which was first, I think, advanced by Corssen but has been developed successively by Bugge (*K. Z.* xx. 2 ff.), v. Bradke (*Z.D.M.G.* xl. 351) and Osthoff (*M. U.* v. 77)¹. It is that I.-E. had the forms *ru* and *lu* as alternatives for the combinations *ɣr* and *ɣl*. The evidence in favour of this is not without its weight, but such a complete inversion—consonant becoming sonant, sonant becoming consonant, and order transposed—is so difficult to account for logically, and so phonetically strange, that one may be pardoned for not accepting it as a final statement of the facts². As far as *lukos* is concerned an easier explanation is possible. From the combinations *ɣru-ɣlu-* the *ɣ* would disappear at once by dissimilation. Of this an excellent example is afforded by the forms of *ἐρύω*, which contains the root *ɣer* combined with the F. S. *ey*, so that we have types of the form *ɣeru-ɣrey*, and *ɣru*³. The first of these is represented by Greek *φέρωμαι*, the second by *ῥύομαι* (cf. *δεικνῦ-* for *δικνευ-* after *δεικνῦ-*), the third by *ἐρύομαι*, where the prothetic *ε* shews that its immediate predecessor had initial *r*, i.e. had lost the *ɣ*. If then the *u* of *lukos* is explicable the loss of the *ɣ* presents no difficulty.

The frequent presence of *υ* in the neighbourhood of an original velar which becomes a guttural in Greek has excited a suspicion that there is some causal connection between them. The prevailing view seems to be that it is the *υ* which prevents the *q* from developing the labial affection, and this appears to be taken by Brugmann (*Grds.* i. §§ 426—429), although on p. 316 he distinctly contemplates the possibility that the causation may have to be inverted.

That the latter is the better view seems to me to follow from these facts. (1) It is not proved that origl. *q* before origl. *u* does not labialize in Greek, and the phonetic probabilities point the other way. (2) In all the certain examples the *υ* is without etymological justification. It is therefore the presence of the *υ* and not the non-labializing of the velar

¹ Cf. also Fröhde *B. B.* xiv. 106 f.

² So P. Persson, *Z. Lehre de Wurzelweiterung*, &c., p. 132 n.

³ Cf. *Cl. Rev.* vi. 58 [p. 126 inf.]. Per Persson's view (*op. cit.*, p. 231, cf. 128) is different.

which has to be explained, and that is only to be done by assuming that the velar *causes* the *v*.

I should therefore re-state Brugmann's "2. Idg. *q, g, gh* = urgr. *k^u, g^u, kh^u*" (*Grds.* I. p. 314)—which in any case should add "*u^k, u^g, u^{kh}*," the labial affection and the explosive being really simultaneous—as follows

2. I.-E. *q, g, gh* = urgr. $\left\{ \begin{array}{l} 1. k^u, g^u, kh^u, \\ 2. {}^uk, {}^ug, {}^ukh, \\ 3. k^u, g^u, kh^u, \\ 4. {}^uk, {}^ug, {}^ukh. \end{array} \right.$

The first and second of these subdivisions then proceed to develope into labials or dentals as he describes; the third and fourth develope respectively into *kv, gv, xv*, and *vk, vg, vx*.

As the rule under this aspect is not yet recognized, it may be as well to give the materials for it, beginning with the accepted etymologies.

These are

1. *νύξ* stem *νυκτ-* = *n^ukt-*. Cf. Lat. *noct-*; Skt. *aktā* (= *ṇqt-*).

2. *γυνή* = *g^un-*. Cf. *g^un* in *μνάομαι*, and *g^uṇ-* in *βανά*.

3. *κύκλος* = *k^{uu}kl-o-*. Cf. Skt. *cakras*, A.S. *hweohl*.

Generally regarded as reduplicated from the root *qel* to revolve, but there is something to be said for Fick's¹ root *qeq* (cf. Skt. *kacate*) meaning primarily "to bend," then "to bend round," "girdle," "encompass."

4. *φλυκτίς, φλύκταινα, οἶνο-φλυγ-* beside *φλεβ* in *φλεψ*.

There has been considerable discussion over this root. It is not easy to gather from the § in the *Grundriss* (I. 427 c), whether Brugmann regards the *v* as generated by the velar or not. Persson (*op. cit.* p. 173 n. 2) has no doubt that he, Brugmann, does so regard it, and holds himself that it should be considered suffixal; comp. *ib.* p. 223. I cannot regard his conclusions as altogether satisfactory, for if his assumption of *φλ-v-γ* enables him to see an exact parallel to *φλυδάω* it only

¹ *Wört.* 4 I. p. 22.

does so at the cost of separation from $\phi\lambda\epsilon\beta$, which cannot possibly be referred to his root $bh\lambda\epsilon\gamma$. In view of $\phi\lambda\acute{\upsilon}\omega$ and $\phi\lambda\upsilon\delta\acute{\alpha}\omega$ the possibility of $\phi\lambda\upsilon\gamma$ cannot be questioned, but the facts seem to point rather to the following arrangement:—

(1) $\sqrt{bh\lambda\epsilon\gamma}$ (whether further to be analysed or not ?) Gk. $\phi\lambda\epsilon\beta$ -, Lat. *fluo* (= *flo μ o* = *fle μ o*), and $bhl\gamma$ -, $bhl^u\gamma$ - in $-\phi\lambda\upsilon\gamma$ -, $\phi\lambda\upsilon\kappa\tau\acute{\iota}\varsigma$ &c.

(2) $\sqrt{bh\lambda\epsilon\mu}$ (probably $bhl\epsilon\mu^{-1}$) weak $bhl\upsilon$ - in $\phi\lambda\acute{\epsilon}\omega$, $\phi\lambda\acute{\upsilon}\omega$ (? = $\phi\lambda\upsilon\sigma\omega$), in Lat. *fluo* (= *flo μ o* = *fle μ o*), *fluvius* (cf. *pluo*, *pluvius*), *flūmen*. With secondary form in $\phi\lambda\acute{\upsilon}\text{-}\xi\omega$, $\phi\lambda\upsilon\delta\acute{\alpha}\omega$ &c. (Persson *op. cit.* 131, Brugmann *Idg. Forsch.* I. 504 n.⁴)

(3) $\sqrt{bh\lambda\epsilon\eta}$. O.N. *bylgja*, Lat. *fluctus*, *fluxi*, for **fulctus*, **fulxi* by the influence of *fluo*, to which they were referred from their sense, aided possibly by the influence of lost perfs. **flexi* from *fluo* = $bh\lambda\epsilon\eta\bar{o}$ and **flui* from *fluo* = $bh\lambda\epsilon\mu\bar{o}$.

5. $\delta\upsilon\nu\chi$ - = (o)*n^ukh*-, cf. the stem *na^xqh*- in Skt. *nakha*-, and *onqh*- in Lat. *unguis*.

The *g* in the Latin word is due to assimilation to the nasal. Persson (*op. cit.* p. 226) regards the *v* in $\delta\upsilon\nu\chi$ - as of the same nature as in $\delta\upsilon\nu\mu\alpha$ beside $\delta\upsilon\nu\omicron\mu\alpha$, in which he is probably mistaken.

6. $\tau\rho\acute{\upsilon}\sigma\sigma\omega$ = *tr^uk-iō* beside $\check{a}$ -*τρακτος*, Müller's *Handbuch* II.² p. 235.

7. $\beta\rho\upsilon\gamma\mu\acute{o}\varsigma$ is connected with Arm. *krēl* by Hübschmann *Arm. Stud.* no. 152, and he makes Arm. $\check{c}$ = I.-E. *q*, but this is not certain.

To these I venture to add the following with more or less confidence.

8. $\kappa\acute{\upsilon}\kappa\upsilon\omicron\varsigma$ is a word of exactly similar formation to no. 3 supra and is from a root *qa^xn* of which the radical meaning may have been "to please." This is nearly retained by Skt. *can*, but has specialized in $\kappa\acute{\upsilon}\kappa\upsilon\omicron\varsigma$ to "pleasing by sight," "brightness."

9. $\kappa\upsilon\lambda\lambda\acute{o}\varsigma$, i.e. *k^ul-iō*- from the root *qel*; so $\kappa\upsilon\lambda\iota\nu\delta\acute{\epsilon}\omega$, &c.

¹ For other possible derivatives of a primitive *bhl*- see Persson *op. cit.* 55, 109, 110, 132, and Feist, *Got. Etym.* pp. 20, 21, nos. 103, 105.

10. σκύμνος = $sk^u m-no-s$ from $s-ga^x m$, cf. $ga^x m$ in Skt. *cam* "to suck."

11. σκύλακες, cf. Hesych. σπάδακες· κύνες (? σπάλακες).

12. κύλα· τὰ ὑποκάτω τῶν βλεφάρων κοιλώματα, Hesych. might be taken with no. 9, but comparing Lat. *oculus* it is more probably k^u-lo- where k^u represents $-q-$ the weak form of $oq-$.

13. The suffixes $υκ$ [$υγ$] are the forms which in Greek are assumed by the suffixes $-qo-$ [$-go$] in their reduced stage. See Brugmann *Grds.* II. pp. 384—387. The Ablaut of root and suffix in declension has not yet been thoroughly mastered, but in all probability certain forms had the suffix in the full stage, others in the weak. Hence by opposite levellings Skt. *vartakas* and Gk. ὄρνυξ. So also ἄμπυξ. κάλυξ needs a word: it is quite distinct from κύλιξ (see infra) and comes from the root *kel*, Lat. *cēlo*, Gk. καλύπτω. καλύπτω however must not be taken as proving original $k^l uq-$: it contains the suffix (or suffixes) $-up-$ also seen in δρύπτω beside δέρω, δορά¹. κύλιξ on the other hand is from the root *qel* of nos. 3 and 9: the suffix $-ιξ$ is not original but for $-υξ$ by dissimilation (cf. *πινυτός*²). Πυνύξ in all probability does not contain this suffix.

14. κωκύνω = $kōk^u iō$ from $qō(γ)q-iō$. Cf. Skt. *kocati*, Lith. *kaukti*.

15. κύνω and κύαρ = $k^u s-$ from $ga^x s-$. Skt. *cas* "to split or cleave."

16. πυκνός may possibly belong to *peq-* in πέσσω &c., if the primary sense was "to knead."

17. ἀμύσσω is possibly to be referred to Skt. *makha-*.

18. γυῖα = $g^u s-ia$ from *gas-* in βαστάζειν and Latin *veru* 'a spit' (on which meat is raised) = *gas-u*. Compare also Gothic *kas* "a vessel" and Feist, p. 64. (Not Latin *gero* = *ges-ō*.)

¹ Possibly also in Lit. *trupis*, 'crumbling,' from the root *ter* 'to bore,' Per Persson, *op. cit.* p. 159. Compare his following pages, whence it appears that he would analyse the

suffix into $-u-p-$. Without going thus far, I would add Lat. *vol-up-* which is to be separated by its sense from *ἐπις*, *ἐλπομαι* (Skt. *varc-*).

² See however *Idg. Forsch.* I. p. 504.

19. γυμνός = *g^um-no-*. The root is *gem*, of βαίνω and *venio*, originally meaning "to move" simply. Any idea that clothes are not a hindrance to motion is based upon imperfect experience of classical over-garments.

20. γύλιος ὁ Ἡρακλῆς, καὶ ἀγγεῖον ὁδοπορικὸν εἰς ἀπόθεσιν τῶν ἀναγκαίων ᾧ ἐχρῶντο οἱ στρατιῶται Hesych. A connexion with βαλάντιον seems possible.

I do not wish to assert that we have here twenty *certain* examples, but I think that those which are certain, with any additional weight which those that are probable lend them, justify the addition of

21. λύκος = *lukos* = *u)l^ukos* = *ulqos*.

The exact conditions which determine this vocalizing of the labial affection (which is clearly a question of syllabism), can hardly yet be laid down: in fact γυνή : βανά : μνάομαι shew that it will not be easily done.

Finally as regards the examples in which the guttural in Greek is alleged to be due to an *u*: the above proof is affirmative, and would not be vitiated by a complete demonstration that *u* could cause *q* &c. to become Gk. *κ* &c., unless the *u* in all the above words is etymologically accounted for. Otherwise all examples of *u* + *κ* &c. = *u* + *q* &c. merely go to shew that a neighbouring *u* was a predisposing cause to produce *u**k* &c., instead of *u**k* &c. But of such examples the only one seriously important is ἐγγύη beside *voveo*, if this be taken as proved. The other example in *Grds.* I. § 428 c, 429 c (γογγύζω beside *βοή* carries its own refutation in γόος) is ἐλαχύς which rests perhaps on less evidence than one would believe, for a much quoted word. As a matter of fact it seems to have originated in Pindar's ἐλαχυνπτέρυξ which contains the root *lqgh-* of ἐλάφρος in the form *e)lqkh^u-*. The form ἐλάχεια then arose as a misreading of λάχεια in two Homeric passages (*ι* 116, *κ* 509) imitated in *h. Hom. Ap.* 197, hence ἐλάχιστος in *h. Hom. Merc.* 573 for ἐλάθιστος but *never* ἐλαχύς in classical Greek. Brugmann has reverted to this point in *Indog. Forsch.* I. p. 504 f., where he appears to throw over the opinion ascribed to him by Persson, that the guttural produces the *u*-vowel. I have

elsewhere endeavoured to shew that his explanation of *κατασβῶσαι* through a form *zgu-* &c. is unnecessary¹, and only mention it here because he adds two further examples, *ύγιής* beside *βίος* and *βουκόλος* beside *αἰπόλος*. As regards the former the derivation seems sufficiently plausible, but as regards the latter the Latin *opilio* makes it probable that *αἰπόλος* and *βουκόλος* are of different origins. Moreover in Doric *βωκόλος* the *κ* is present although *υ* does not precede.

IV. Latin.

1. *lupus*. The accepted explanation of this is that it is a borrowed word, probably from Sabine, as it is referred to *υλqo-* and *q* does not become *p* in Latin. For reasons given below I am far from being convinced of that rule, and it seems to me highly improbable that a word of this kind should have been borrowed. Names of unfamiliar objects and those peculiar to certain localities are adopted, but it is absurd to suppose that the wolf with its legendary place in Roman history should come under either head. Besides *lupo-* gives I.-E. *lupó-* exactly.

2. *vulpēs*. No amount of ingenuity will equate this with *ἄλώπηξ*, whereas with the frequent Latin change of class², and the *p* as a representative of *q*, it suits well with *υλqos*. As regards the latter change, it may certainly be a borrowed word, but one fact bearing on the labialism of velars in Latin does not seem to have been taken into consideration. It is this: the abbreviation for the district of Rome called the Subura was *SVC* i.e. *sug-* as we know from inscriptions, the testimony of Varro (*L.L.* v. c. 2), Quintilian (I. 7. 29) and Festus (p. 309 Müll.). Now if this were merely evidence that the name *Subura* was derived from a root containing *g*, it might of course be replied that the district was inhabited by Sabines, and they brought their own name with them; but it does more than that, it proves that the change took place at Rome between the introduction of writing, say 500 B.C., and classical times, and if there were two different sets of phonetic changes going on in different districts of Rome, why need we

¹ *C. R.* vi. 277 [p. 106 infr.].

Vulpus not before George Meredith.

be surprised at any number of irregularities in Latin? *Subura* however does not, while *vulpes* does, conflict with Bugge's view that *g* became *b* only before original *u*. (*B. B.* XIV. 59 f.) His alternative suggestion, that the variation depends on the position of the accent, is I think much less likely. Compare Hoffmann's theory that I.-E. *qʷ* became Latin *p* (*B. B.* XVIII. 149).

V. Gothic.

1. *vulfs*. In view of the constant assimilation of *χ* to *f* by a neighbouring labial, this may be regarded as regular for *ʷlqos*. The other root seems to have no representative.

VI. Lithuanian.

1. *vilkas* is regular.

2. The only difficulty with this language is that Fick, *Wörterbuch*³ II. 249, H. D. Müller, *B. B.* XIII. 315, and even Brugmann, *Grds.* II. p. 237, give a Lithuanian form *lāpe* as cognate with ἀλώπηξ. This I cannot understand, as, apart from the other difficulties, Lithuanian *ā* does not appear to equal either *eʷ*, *oʷ*, *ōʷ*, or *u*.

In conclusion I should mention that, although mine differs from it, there is an interesting similar attempt to classify these words, by Mr T. C. Snow in the *Transactions of the Oxford Philological Society* for 1884—5, p. 18 f.—that H. D. Müller *l.c.* also identifies *lupus* with *lopāsa-* (so also Moulton, *Proceedings of the Camb. Phil. Soc.* May 22, 1890) and connects λύκος with λύγξ—and that Fick, *Wörterbuch*⁴ I. p. 135 also analyses *ʷl-qo-* but makes the root *ul-* or *ʷel-*, 'to howl,' cf. Gk. ὕλακτέω.

My attention was drawn to the word ἀλώπηξ in August 1891 for reasons which made it necessary to consider its cognates, and in particular its relation to *alowēs*. The result in what is practically the above form was written down at that time. Unfortunately the digression, which could hardly

be avoided, on the groups *υκ* &c. in Greek, led directly to two topics—the guttural series, and pre-Greek accentuation—which if followed up would completely have swamped the main subject, *Fox and Wolf*. I have therefore allowed the form of my paper to remain essentially unaltered, but it may be as well for my own credit, and for the benefit of casual readers, to protest against any idea that the rule propounded on pp. 194 ff. is there *proved*. Elsewhere¹ I have stated my belief that to prove a rule there are three things needful. 1. Derivations otherwise exact. 2. The precise conditions stated. 3. All examples to the contrary disposed of. I have given the positive evidence; the conditions most probably depend on the two (or one of the two) important subjects just mentioned; and the examples to the contrary cannot be ascertained till the conditions are known.

I add two remarks which would have found their place, had the paper been re-written.

1. If my position as regards 'descriptive' nouns is good it seems to follow that all nouns which defy this analysis are to be classed in one of three groups: (a) borrowings, (b) isolations, i.e. all cognates have accidentally been lost, (c) survivals from a more remote period. Of the former two I have no examples to hand; of the third, words like *πῦρ*, *fire*, the pronouns, and perhaps the numerals may be cited. Such a remote period would not be very far removed from the interjectional state of language, and implies a lower degree of mental development than even the formation of 'descriptive' nouns. *These words are moreover suffixless*, and therefore the morphological evidence points also to their greater antiquity.

It must not be thought that this cuts away the ground for fixing the primitive home of the race, because it postulates a period anterior to the 'descriptive,' while the words like "beech" &c., on which so much weight is justly placed, are clearly themselves descriptive in their origin. By the Indo-European language is meant the language as it was spoken (or rather, what we can reconstruct of such language)

¹ [See pp. 121 f. and 143 inf.]

before the dispersal of the race; and by the home of the Indo-Europeans is meant the *last* position they occupied before the dispersal began.

2. Bechtel, in his *Hauptprobleme*, p. 113 f., discusses some of my examples of Greek ν , and gives another explanation. He believes that ϑ becomes ι in Greek, and that under similar circumstances (not defined) ν is preferred in the vicinity of a velar or a labial (of labials producing it he adduces no examples). If his treatment of ϑ were accepted throughout, the whole system of the modern school crumbles about their ears, and in this particular case his best examples for $\iota = \vartheta$ are all capable of other explanations. Perhaps then *my* empiric rule that the ν belongs to the velar may be allowed to sweep away the support which *his* would receive from $\kappaύκλος$ &c. Of course his theory leaves the loss of the initial consonant in $\lambdaύκος$ out in the cold, and he would probably adhere to v. Bradke's explanation.

(7) ON THE FORM *κατασβῶσαι* HERODAS v. 39.

(Classical Review, June (p. 277) 1892.)

κατασβῶσαι (HERODAS v. 39).

PROBABLY few purely classical scholars think it necessary to study a specialist periodical like *Indogermanische Forschungen*. It may therefore be worth while to draw their attention to the fact that the above form receives an exhaustive discussion and defence from Dr Brugmann in the last number issued (vol. i. p. 501). His argument is this: $\sigma\beta\epsilon\sigma-$ is to be analysed into $\sigma\beta-$ root and $-\epsilon\sigma-$ suffix (cf. $\tau\rho-\epsilon\sigma-$: $\tau\rho-\acute{\epsilon}\mu\omega$, $\xi-\epsilon\sigma-$: $\xi-\acute{\upsilon}\omega$) and so $\sigma\beta-\eta-$ (cf. $\pi\lambda-\eta-$, $\pi\rho-\eta-$, &c.). Then just as we frequently find a suffix $-\omega-$ in exactly the same function as $-\eta-$ (cf. $\gamma\nu-\eta-$: $\gamma\nu-\omega-$, $\xi-\eta-$: $\xi-\omega-$ &c.) so here we may analyse $\sigma\beta-\omega-$. Or, on the other hand, it is possible that *κατασβῶσαι* represents *κατασβόησαι* in which case we may suppose that beside $\sigma\beta-\epsilon\sigma-$ existed $\sigma\beta-\omicron\sigma-$ with an alternative form of suffix, in support of which are cited the Hesychian glosses $\zeta\acute{o}\alpha\sigma\omicron\nu$ $\sigma\beta\acute{\epsilon}\sigma\omicron\nu$ and $\zeta\omicron\hat{\alpha}\varsigma$ $\sigma\beta\acute{\epsilon}\sigma\epsilon\iota\varsigma$ which are supposed to contain the root-form *zdos*.

It is scarcely necessary to inform even the uninitiated that the harlequin of these transformations is the velar guttural, but they may be pardoned for regarding with some suspicion a *science* which, after laying down as *laws* that the velar guttural becomes a labial before *o*-vowels and a dental before *e*-vowels, is able by its principles to accept recorded forms *zbes-*, *zdos-*, as confirmation of those laws. However so strangely powerful is the attraction which a subtle proof exercises over the greatest minds, that Dr Brugmann prefers this explanation to his former one. His reason is that $\sigma\beta\omega-$ is unattested: but he has no difficulty in assuming $*\sigma\beta\omicron\sigma-$ to explain $\sigma\beta\epsilon\sigma-$ and $*\zeta\epsilon\sigma-$ to explain a doubtful $\zeta\omicron\sigma-$.

For my own part, if I were compelled to choose between these explanations, I should prefer the assumption of *σβω- to Mr Rutherford's κατασβέσαι, but if I am not mistaken a still simpler explanation of κατασβῶσαι will already have occurred to most scholars.

The aorist ἔσβεσα is comparatively speaking isolated. The only aorists which would naturally form with it a mental class (Paul's 'formale gruppe' in its narrowest sense) are ἡμφίεσα, ἐκόρεσα, ἐστόρεσα¹. The last-named is common in Homer, is found in Trag. and Theocr. Beside it exists the first aorist ἔσπρωσα, Trag. and Theocr., with meaning, as far as we can judge, absolutely identical, and the use of both stems is continued, as may be seen from Veitch, by later writers. Now, under these circumstances, is it not *possible* that Herodas or his contemporaries using in daily speech, or at any rate finding in their poetic models, the forms στορέσαι and στρώσαι used indifferently should venture on the analogical formation κατασβῶσαι² as equivalent to κατασβέσαι, and is it not more *probable*—whether we assign his date to the third century B.C. or agree with Mr Ellis in placing him after Vergil—than that a form σβω- or σβοσ- should have come down to him from Indo-European times without having once come to the surface in literature? Homer and post-classical authors demand different treatment in the matter of etymologizing³.

¹ I take these from Krüger § 39, Taf. viii.: the first may almost be excluded.

ten lines below.

³ Bücheler's analogy of πλώσαι lacks a middle term.

² I need scarcely point to ἀκήκουκας,

(8) SOME LATIN ETYMOLOGIES (*altus, colo, iubar,*
numen, scio).

(Read before the Cambridge Philological Society on Feb. 9, 1893,
and published in brief in *Camb. Univ. Reporter*, Feb. 14, 1893.)

SOME LATIN ETYMOLOGIES.

MAKING derivations is a very easy pastime, and I should not consider my having a few new guesses to propose, to be, in itself, a justification for taxing the attention of this Society. My desire has been in selecting these few words to draw attention to what I feel to be a matter of some importance for philology in general. Fifty years ago it would have seemed utterly absurd to demand that the meaning of words should be taken into account for etymologies, because etymologists proceeded on very little else. It is true their ideas of what senses could pass into one another were somewhat lax, but there was no Digamma in the category of meanings and accordingly transformations of sense were not so easily produced as transformations of sound. Of late years as we all know the laws of phonological change have been pushed to a great pitch of accuracy, but at the same time by the aid of judicious theorizing they gradually have become extremely wide. A balancing principle became necessary and this was applied by insisting upon equal strictness in morphology. This however exercises only a weak control, as formative principles are comparatively few in number, and therefore I feel it not unnecessary to point out that strictness of rule in tracing sense is as important as in phonology. It is true that at present there is scarcely any consensus of opinion as to what is legitimate developement of meaning, but every false developement that is exploded and every true developement that is added helps to provide material for rules. The remarks which follow all bear more or less directly upon this point¹.

¹ [Compare the fragment on *Semasiology* in Part II. below. C.]

(1) *altus*—'high', 'deep'.

This word is usually supposed to be identical with the past participle of *alo*, a view which would seem to be shared even by Brugmann. It appears to be implied in the *Grds.* II. pp. 207, 218. Gothic *alpeis* and *alds* are also supposed to be from the same root, so that we have to suppose the developement of 'nourished' into 'high, deep,' for one language and of 'nourishment' into 'age' in another. This may be possible but is, I venture to say, not too probable. The fact of *altus* meaning both 'high' and 'deep' is very much against it, for if the past participle did take on the successive senses of 'grown,' 'well-grown,' 'full-grown' and 'lofty' there is no step at which the developement of 'deep' could begin and the ideas of 'height' and 'depth' are mentally contrasted not associated. The sense therefore definitely points us to some derivation which shall start from the common characteristic of height and depth, namely "*vertical extension*."

Now consider *lātus*. *lātus* implies lateral extension and by modern laws may be etymologically a doublet of *altus*. On this point there is some difference of opinion. Brugmann and Osthoff would derive both *al-* and *lā-* initial from the same original sound viz. $\bar{l}$, while Bechtel and Mr Thomas would regard *lā-* as a compound of the weak root and the suffix *-ā-*. This is a matter of indifference to the present argument: in either case both *altus* and *lātus* go back to a root of which the weak form is *l-*. Now ideas of relation—among which may be placed that of extension—naturally belong or tend to fall into that class of words which are called pronouns: they are also closely allied to comparatives and superlatives. Therefore instead of regarding the *-to-* suffix as *participial* I assign it to its other great recognized use and make it superlative in *altus* and *lātus*: while the root is obviously to be found in the more or less pronominal *ultra*, *ultro*, *ōlim*, *alius* and *alter*, all of which express the main idea of *separation*. Now if the original sense was that of "great separation" it is quite consonant with linguistic developement to find one of these closely kindred words developing the sense of "great vertical separation" and the

other than that of "great horizontal separation", or that in another language the separation should be that of time: for which indeed cf. *olim*. I am glad to see that Feist whose views upon sense-developement are much more sober than those of the average etymologist is also of opinion that the reference of *old* to *alo* is untenable (*Got. Etym.* p. 6).

(2) *colere*.

colo is unhesitatingly derived from the root *qel* and so is referred to Skt. *carāmi*. This is an excellent example of what may happen in the way of transition when the senses of the Latin and the Skt. word are compared. "Dwelling in" and "wandering about" are dissimilar and hardly to be explained by a social change from a nomad to an agricultural life. But in Latin itself the various meanings are extensive and dissimilar. It is true that we are so habituated to regarding the ideas as related and our own language is so full of derivatives from all the senses that there seems an inherent connexion between *cult*, *cultivate* and *culture* and of all with "inhabiting". Generally speaking, however, Latin words develop their meanings along fairly narrow lines, and those which shew wide diversity of sense should excite suspicion that two distinct roots have given formally identical results, which from the general tendencies of Latin sound laws is a thing that may easily happen. A further indication of this in the case of *colo* is the fact that the past participle *cultus* is not directly traceable to *qlto-s* which should give *quoltus* in Ciceronian Latin (*Brugm. Grds.* I. p. 325, § 432 Anm. 2).

I think these indications taken together are almost sufficient to justify from Latin alone a conclusion which I had reached from evidence outside Latin, viz. that there existed in Indo-European two distinct roots each of which would give *colo* in Latin. One of these roots is of course the *qel*- of *κύκλος* and Skt. *carāmi*; the other has not yet I think been shewn to exist: its form was *qyel*- and its sense was as emphatically 'dwelling' or 'abiding' as that of *qel*- was 'motion'. This root justifies its assumption by the number of difficulties it explains. In the

first place a plain derivative of this root is to be seen in Skt. *kula-* R. V. *kulapá-* "head of the family", *kulāyayát-* 'dwelling' or 'nesting in', *kulāyin-* 'nest-forming'. The derivatives I think make it probable that the sense of the primitive *kula-* was first 'home' and then 'household' instead of vice versa, as Grassmann seems to suggest. The R. V. word *kulyá* a 'stream' 'river' lacks a derivation so far as I know, and remembering "the remarkable way in which streams flow past human dwellings" (to modify an old jest), it is possible that *kulyá* also may be from this root. Turning to Greek the existence of the two roots is clearly marked. One of the most serious objections which Dr Fennell has raised to the theory of dentalizing velars in Greek lies in the verb *πέλω*. He justly says that *ἀνατέλλω* is no proof that the form once was *τέλω which became πέλω under the influence of πόλος ἔπλετο, &c. I do not indeed consider this assumption impossible from the formal point of view, but the sense of 'motion' is so entirely absent from πέλω that the derivation from *q̣melo is surely preferable. q̣m would naturally be expected to become ππ in Greek and initially π-. We thus set -τέλλω περιπλόμενος πόλος &c. on the one side from q̣el with the full sense of motion against πέλω &c. on the other from q̣mel with the full sense of rest. But this is not all: it may be doubted if any Greek word has given more trouble than πόλις, and simply from the ancient domination of Sanskrit. If Greek alone had been considered τρόχις comes from τρέχω, στροφήις from στρέφω, τρόπις from τρέπω and why not πόλις from πέλω? But as πέλω "I dwell" came from carāmi 'I move from place to place' and as πόλις must necessarily be pūr 'a fortified place', πόλις and πέλω have been kept apart even at the cruel cost of admitting an "irregular representation of a long sonant liquid."

By deriving πόλις from q̣mol-i-s we not only escape that pass but we see the cause of the doubled consonant in πτόλις which has hitherto been unexplained¹.

It may be noted that in ἀμφίπολος it is impossible to say

¹ [I do not know how Mr Darbishire perhaps be found in varying conditions of sentence-sanddhi. C.] meant to explain the doublet. The least objectionable expedient would

which root is present, as either sense is suitable, but the preference seems to lie with the sense of 'motion'.

In Latin if Hoffmann B.B. xviii. 149 is right in his supposition that I. E. *q̥* becomes *p* we should expect a present **pelo* of which the past participle would be *cultus*. Such a dissimilarity is of course too great to maintain itself and **pelo* if it ever existed has disappeared while *cultus* drove out *quoltos*. I believe however that **q̥elo* no less than **qelo* could become *colo* directly.

(3) *iubar*.

That *iubar* is from *iuba* is a proposition as old as Varro (L.L. vi. 2, p. 53 (45)) and as new as Mr Wharton. It seems a pity to disturb it merely on the ground of insufficiency. If a comet has a tail why should not the sun have a mane—or even Phosphor to whom Varro applies it?

The fact however remains that in Latin the constant and consistent sense of *iubar* is 'effulgence', 'radiance', 'dayshine' and this sense is given exactly by analysing *d̥iu-bhas-*. It thus contains two common I. E. roots *d̥iu-* as in *Zeὺς Iu-piter* and *dies*, and *bhas-* as in Skt. *bhāsati*, perhaps Lat. *harena*. For similar root nouns compare *tibicen* and Brgm. Grds. II. § 163, p. 462.

(4) *numen*.

Numen is, I think, another example of what I would call sense-syncretism. It has two perfectly good etymologies and the two together exactly cover all its meanings. The connexion with *nuo* was of course obvious and was probably consciously present to the Romans themselves. At all events Varro has it. But *numen* is also the exact equivalent of *πνεῦμα* and both represent an original *qney-men-*.

The root *qney* is in all probability a secondary root formed with the suffix *-ey*. The simplex then will have the form *gen-* which is probably to be seen in Skt. *kan-*, *can-* 'to be pleased, take delight in'. The same root and suffix but with different

Ablaut viz. *geny-* is to be seen in Gothic *-ginnan* if Bugge's view is right that this is connected with O. Bulg. *po-čina* (P. and B.B. 3. 405). The primitive sense will then probably be 'inspire' of the mental, 'set in motion' of the physical, range of ideas.

(5) *scio*.

I do not think any even moderately convincing etymology of this word has yet been proposed. Mr Wharton is actually forced to agree with Lewis and Short and connect it with Gk. *κείω*. I think anything more improbable than that the Latin genius would develop the idea of 'split' into that of 'know' can scarcely be conceived. A metaphysical language like Skt. offers no safe analogy. Still perhaps Fick's (*Wört.* I' 143) connexion of it with Gk. *σκιά* and Germ. *scheinen* is even worse.

I think these etymologists have been led away by seeking for an *i*-stem. This is not necessary: by Latin rules the verb ending *-iō* becomes *-io* after a consonant. If it was so in this case, we have in *sc-* clearly the weak form of some root, and to the best of my belief there is no reason against its being the weak form of the root *seǵh-* which we have in Skt. *sah* and Gk. *ἔσχω*. When the breathed *s* and the voiced *ǵh* came together by the expulsion of the vowel, assimilation necessarily took place, and for Latin the breathed sound would naturally conquer. *scio* then is simply 'I grasp'.

2. SELECTIONS¹ FROM OCCASIONAL WRITINGS.

- (1) From a notice of Wharton's *Etyma Latina* (1891).
- (2) From a notice of Fennell's *Indo-European Vowel-System* (1892).
- (3) Abnormal Derivations (1892), an addition to (1).
- (4) From a notice of Sweet's *English Grammar* (1892).
- (5) The Göttingen School of Comparative Philology (1893).

¹ These selections only include such passages as contain definite expressions of Mr Darbishire's opinion on

points which seem of permanent importance, and so much of the context as was necessary in each case. C.

EXTRACTS FROM A REVIEW OF WHARTON'S
ETYMA LATINA

(*Classical Review*, May 1891).

IF we assume, as we are bound to do in default of some definite statement, that Mr Wharton accepts the methods of Brugmann, Paul, Osthoff and other names which will be found in the list of authorities, what are we to say of this book? It undoubtedly shows much labour; contains useful hints, not few even brilliant derivations; and yet it must be condemned.

The form of the work is unhappy. Latin is and always will be the despair of scientific philology for reasons which are obvious: in the first place what we are told of the origin of Rome is strongly against any original purity of stock, so that, as Mr Wharton would put it, they must all have begun by talking 'dialectically'; and in the second place, our records are doubly imperfect—imperfect in that we have, comparatively speaking, *no* archaic records, and in that the literature on which we mainly rely is as far from representing the spoken language of the people as any literature could well be. If then—provided that no new storehouse of information becomes accessible—the rules of scientific philology ever reduce to order all the words in the Latin dictionary, it will mean that the rules themselves are so elastic as to be valueless. The extent of our ignorance could not be more clearly exposed than by adopting the dictionary form, and consequently the temptation to wild conjecture is great. We are told in the Preface that 3055 out of the 4320 Latin words which do not 'sufficiently explain their own forma-

tion' are 'treated' in this work; there could not be a better illustration of our remarks above than the fact that with all Mr Wharton's ingenuity, of which specimens will be given below, over 10 per cent. of these 3055 have to be 'treated' in silence. About 450 are derived from Greek, under 50 with some plausibility from other languages, and about 100 on the assertion of Roman grammarians (which counts for nil on such a point) or with a mere pretence at reason, as when *buxus* is traced (through *πύξος*) to Paphlagonia because Catullus has *Cytore buxifer*: and *rosa* is given as Oscan because Vergil has *rosaria Paesti*. So *Gallicis cantheriis* relegates *cantherius* to Gaul, *Maurorum attegias* makes the latter word Moorish, and the Celtic origin of *petorritum* assigns all names of wheeled vehicles to the same language. Nay, we have to go to 'some Celtic dialect' (which is scientific but vague) for *quadru-* and *quadra*, the reason being that they conflict with a rule of Mr Wharton's that *d* before *r* becomes *t*. Parenthetically we may here observe that most of the derivations by which this rule is supported may be considered matters of opinion, but not when we find *nutrio* ascribed to a root *NĒD* in *νήδυμος* (!): we think most readers will prefer the normal analysis *νη-δυ-μος* from the root of *δύη* &c. with the negative prefixed¹. The testimony of grammarians to the origin of a word is accepted or not according to the urgency of the case: thus under *lanista* (of which we may in passing notice the brilliant identification with *danista*) we find

'Isid. calls it Etruscan, only because the use of gladiators came from Etruria';

but under *andabata* 'Gaulish (cf. *Gallus* for *mirmillo* Fest.)?'

Perhaps however most discredit will be cast on philological method by Mr Wharton's too lively imagination both on the treacherous ground of analogy and in tracing connexions in sense. As regards the former it may be possible to believe that *posca* owes its termination to *esca*, but who can credit the statements that *marmor* has the ending of *aequor*, *celox* (= *κέλης*) of *velox*, *autem* of *septem*, *spinter* of *tuber*, that *caepe* is 'quasi

¹ [Or to read everywhere with Leaf and others *νήδυμος*. C.]

Adj. Neut. (like *turpe*)' and the like? This dangerous weapon is even employed without necessity, as when *facilis* is pressed in to account for *dapsilis* which, like *facilis* itself, is only another example of the absorbing power of *-i-* adjectives in Latin as a class. As for the unexplored region which rejoices in the name of semasiology, we fear that no system can be elastic enough to admit such hypotheses as the following. *Norma* 'a square' is derived from *nonus* 'ninth' as being 'shaped like L the 9th letter in the Etruscan and Faliscan alphabets.' Now, to begin with, the essential part of a 'square' is its right angle: the angle of an Etruscan L is anything but a right angle. Nevertheless a Roman preferred to use it as a simile, and, lest he should make himself too clear, he simply described the letter by its numerical order in a foreign abecedarium, leaving it to the hearer's ingenuity to work out the connexion. There is no other example to vie with this, but one or two others must be quoted: *parra* 'a bird' (surely a misleading translation) is from '**parsa* a companion' and goes with *parricida* which is thus reduced to an infraction of the game-laws: no mention is made of Fröhde's convincing equation of the first element in *pāricida* with the Homeric *πῆποι* 'kinsfolk.' Sometimes a needlessly tortuous sense-connexion is traced, as when *bracchium* is accounted for as being 'shorter than the leg': it of course denotes the fore-arm which is shorter than *lacertus*, the upper arm. Again the derivation of *patro* from *pater* (which had occurred to me independently) is spoilt by the far-fetched explanation of *pater patratus* 'the father who acts as such.' It really means the *pater* who had a *pater* and so excluded all but free-born citizens (of two generations, if *pater* be taken = senator).

Before concluding this notice it may be well to explain why we condemn this attempt, when no better is forthcoming and when scientific methods confessedly yield so imperfect results. The reason is that making etymologies is no more the end of scientific philology than making new fossils is the end of geology. New etymologies, if sound, are welcomed, but it is not for themselves, nor even for the laws to which they lead, but for the proof of those laws, *i.e.* the reason which underlies

them. Mr Wharton, by many of his ingenious derivations, does give some new laws, but without any attempt at proof and in the republic of science ipse dixit even backed by the highest reputation are not current coin. Thus the rule that original 'pretonic' *e* becomes *a* in Latin has a very good *prima facie* case made out for it by the examples, but no proof, and we must moreover take it, on Mr Wharton's assertion, that the accent was pitch, despite all current theories upon Latin accentuation.

All this, as well as the statement (also *ex cathedra*) that the *Ursprache* had a *ü* (short and long) and that the *Ursprache*¹ was once spoken, bears out the hypothesis which we mentioned at first, that Mr Wharton belongs to a different school from that of Brugmann and Paul. If this be so, we can only apologise once more for treating his book according to our lights.

FROM A NOTICE OF FENNELLS *INDO-EUROPEAN VOWEL-SYSTEM*.

(*Classical Review*, Feb. 1892.)

THIS is one of the most striking contributions to classical philology which has appeared for some time. The originality of Dr Fennell's views and the freshness of his ideas are only equalled by the pertinence of his attacks on the weak points of his opponents, among whom he is disposed to reckon all Germans. This is scarcely surprising. Dr Fennell's services to philology have not received proper recognition; the famous sonant-nasal theory was to a great extent anticipated by him, and as early as 1873 he published a refutation of Curtius' then reigning principle of 'phonetic ease.' He is therefore naturally indignant at these being hailed as new discoveries by foreign

¹ [i.e. spoken as a single, uniform language; Mr Darbishire means that it must have been more or less broken

by dialectic peculiarities even at a fairly early stage of its development. C.]

scholars, and thus the 'Neugrammatiker' find small favour in his eyes. We fear that some readers trained in the methods and phraseology of his opponents will be disposed to look down on this work as belonging to an exploded state of the science, but they will be wrong. Where Dr Fennell rejects the discoveries or the methods of the new school he does so with his eyes open. His antagonism is of knowledge and not of ignorance.

The pamphlet before us begins with an explanation of those exceptions to Grimm's law which have hitherto been associated with the name of Verner. Dr Fennell rejects the theory that accent is the disturbing influence and substitutes for it syllabism. The coincidence between the observed facts and the accentuation in Sanskrit he proceeds to account for by the hypothesis that accented syllables in Indo-European contained as little 'consonantal sound' as possible. Thus, taking two well-known examples *bhrāter-* and *māter-*, in the former case the accentuation is *bhrā-ter-*, therefore the syllabism is *bhrā-ter-*, therefore Teutonic has *þ*: in the latter case the accentuation is *matér-*, therefore the syllabism is *māt-ér-*, therefore Teutonic has *d*. It is obvious that this hypothesis gives us a canon by which to determine I.E. syllabism and this Dr Fennell next proceeds to do, setting out the general rule and the exceptions which are to be made. From this he passes to the main topic of his paper, the vowel-system in Indo-European. Here he propounds a new explanation of the 'light' diphthongs (*ei*, *ey*), that they were developed from *i*, *u*, by accent, and proceeds to construct a classification of vowel-sounds consisting of four 'keys' (corresponding to what are called 'scales') each containing four major, further analysed into ten minor, subdivisions (roughly corresponding to 'stages'), and the paper concludes with a discussion of some exceptions and difficulties.

The proposed alternative for Verner's law is a brilliant hypothesis and certainly deserves thorough investigation. The statement in its present form is too brief to be satisfactory. As regards the vowel-system we fear that Dr Fennell is not sufficiently clear to escape misconception. After his own system he proceeds to print Brugmann's 'Ablautsreihen,' as if they

were to be contrasted. The two systems really supplement each other. Dr Fennell's 'keys,' resting as they do almost entirely on pitch-accent, are in fact an elaboration of the little-worked hypothesis that the *e* : *o* variation &c. resulted from variations of pitch. As a tentative expansion of this hypothesis (which it can very well bear) his work will be found valuable and in no way conflicting with that of his enemies. His error lies in claiming too much for his own client, pitch-accent, and in not recognizing sufficiently the effects of stress. Thus the accent which could cause to vanish the vowel of the preceding syllable must have been stress: on the other hand we do not think there is the slightest ground for maintaining, as he does (p. 26), that stress could lengthen a vowel. In point of fact however many controversial topics suggest themselves which it would have been impossible to treat in his limited space. As it is, his work suffers greatly from compression and we hope soon to see this pamphlet made the basis of a thorough-going examination into all the evidence both positive and negative. He has himself given us in the last number of the *Classical Review* (vol. v. p. 451) a fuller statement of his views on the sonant nasal, which are here merely referred to in a foot-note.

We fear that we differ widely from Dr Fennell in our conception of fundamental principles. Such sentences as: 'It is obvious that a vowel is more liable to change in a syllable which ends in a consonant than in a syllable which does not end in a consonant' (p. 6)—'The change of aspirates in Greek in (*sic*) *mediae* or *tenuae*, as in *λαμβάνω*, *σπρόμβος*, is more explicable at the end than at the beginning of a syllable' (p. 11)—'Sanskrit *k*, *g*, *gh*, were respectively changed to *ch*, *j*, *h*, owing to lax pronunciation of the consonantal part of an accented syllable' (p. 13)—'ἐπλε is clearly poetic and also optional' (p. 17)—all seem to rest on the individualistic hypothesis that language is the property of the speaker, which he can modify and change at will. If this were so, so were it uttered, but 'tis not so, and 'twas not so, and God forbid it should be so, for the sake of the science of language.

The issue as regards accent is so all-important that we must

re-state it at length. Dr Fennell's own words are: 'I have to defend my assumption that stress is to be regarded as distinct from accent' (p. 26). This is not the point. The distinction between stress and (pitch) accent is quite recognized and there is no confusion in thought, however the words are used. What must be defended is the assumption that the Greek accentual system, which was undoubtedly pitch, and which reaches us only from Alexandrian grammarians, can be taken as faithfully representing the Indo-European accent-system, although it conflicts with the testimony of every known language of the family. The remark on p. 32 'The terms *προσφῶδια* and *accentus* clearly referred to the pitch of the voice' is obviously only intended to defend the use of the word, and not to prove the fact in issue.

On the other hand Dr Fennell is distinctly in the right as against 'the German school' when he denies that diphthongal forms were 'prior to those containing *I* and *U* alone' (p. 18). This might appear fairly obvious; but as the contrary has been asserted quite recently by so distinguished an exponent of modern views as Dr Wilh. Streitberg, a few words of refutation are advisable.

Dr Streitberg's own words are: 'herrscht doch darüber meines Wissens allgemeine Uebereinstimmung, dass die Vokale *e/a/â/o/* und die ihnen entsprechenden Längen—die sog. Vollstufenvokale also—die einzigen Sonanten oder silbischen Vokale des Indogermanischen waren zu einer Zeit, als die Schwundstufe sich noch nicht ausgebildet hatte,' and again, 'Wir haben also prinzipiell für alle Silben, haupttonige wie nichthaupttonige ursprünglich einen der vier Vollstufenvokale anzusetzen.'—*Indog. Forsch.* I. p. 84. This illustrates the danger of working philology on algebraic methods, and studying symbols until principles are lost sight of. The assumption of a still more primeval epoch than any reached by logical inference, of which it can merely be predicated that stress-accent did not exist—for we defy Dr Streitberg to point to any other distinctive characteristic of this period—is not only unnecessary but even harmful. It is unnecessary, for the sole test of merit in a hypothesis is the number of facts it explains, and this explains none. It is harmful,

because it obscures the important fact that there is a priority, but it is logical priority, not chronological. It is quite correct to speak of a root *lei*q and of a suffix *es*, of a root *uel* and of a suffix **qe*, of a root *yer* and of a suffix *eu*, because whenever any one of these roots or suffixes was presented vividly to the consciousness it would assume that form. But it is not correct to speak of **leiqes*-, *uelqe*- or *yereu*- as bases, or, as Dr Streitberg does (on p. 89), of **esent*(*i*)¹ for this reason. It agrees with all that we know of stress to suppose that it in Indo-European as elsewhere coincided with emphasis; but the conditions under which two consecutive syllables are emphasized are rare indeed. Either then the emphasis and therefore the stress is on the root, or it is on the suffix, but not on both. Presumably it is because Dr Streitberg perceives this that he is compelled to throw his **esenti* back into a time anterior to stress, but then other considerations arise. Was there an intervening period of pitch-accent influence or did pitch and stress simultaneously dawn upon a hitherto accentless people? In either case we ultimately arrive at something dangerously similar to the exploded *Lautspaltung*, simply because we insist that sound-analysis shall have its historical analogue. There is no need to assume any succession of epochs for the changes in question; when, and as soon as, a complex was formed from any two of the above simplicia, the mental act at once subordinated one or other of them, and produced *lei*q-s- or *liq*-es-, *uel*-q- or *ul*-qe-, *yer*-u- or *yr*-eu- (to say nothing of the more subtle modifications of pitch). These forms have absolutely no priority over one another; they would all exist, potentially at least, at the same time, although not *in the same mind at the same time*. It is rather late in the day to have to assert that language does not reflect the mental growth of the individual, but this is what it really comes to. We repeat then that we are not justified in assuming any period prior to the I.E. period—meaning thereby that in which stress-accent produced its changes—except the root-period, and our knowledge does not yet justify us in assuming that.

¹ And even by his own showing, this ought to be *esent*(*ei*)!

We conclude this paper with the hope that Dr Fennell will not consider us presumptuous in congratulating a scholar of his name on his most stimulating and refreshing pamphlet. We trust it will be widely read and have the desired effect of calling attention to the exact evidence on which our conclusions rest. *Νᾶφε καὶ μέμνασ' ἀπιστεῖν* is still a good rule, and neglect by German 'authorities' of the first injunction does not justify their English followers in losing sight of the second.

ABNORMAL DERIVATIONS

(*Classical Review*, April 1892).

THE precise canons which are to decide whether a derivation is or is not justifiable do not yet appear to have been laid down with sufficient strictness; nor is the task an easy one. Much must always depend upon the logical equipment of the etymologizing mind. At the same time I do not despair of seeing certain broad rules formulated which may not be transgressed, and in the faint hope of contributing to this desirable result, I proceed to urge a few objections to a derivation proposed in the last number of this *Review*.

I think I may fairly presume that Mr Wharton's paper on p. 11 [of *Class. Rev.* Vol. vi.] is elicited by my reference to his derivation of *norma* in Vol. v. p. 219 [p. 121 *sup.*]. It is only right therefore to admit that—quite apart from the evidence he brings—if his derivation in *Etyma Latina* had rested on the same reasoning as he now adduces I should not have referred to it so slightly. Then he said in effect: '*norma* by Latin phonetic laws may mean "ninth," *l* was the ninth letter in certain non-Latin alphabets, therefore *norma* can = *l*'; or this:

'*l* was the ninth letter of the genuine Faliscan alphabet, later Faliscan inscriptions in the Latin alphabet have a right-angled *l*, therefore the Latin name for a right-angled instrument was "ninth"—and the latter line of thought he does not seem to have quite deserted. But *now*, he also asserts that *l* was the ninth letter of the Latin alphabet and therefore **nōnima* 'ninth' could mean *l* and so be applied to an l-shaped instrument. We now enter the domain of argument and it is necessary to examine the sequence of the propositions and the evidence to support them.

The three points which Mr Wharton proposes to prove are not logically sufficient to establish his conclusion: it is necessary for him to prove (1) that *l* was the ninth letter of the Latin alphabet, (2) that at the same date it contained a right angle, (3) that subsequently *nm* became *rm* in Latin, (4) that the change of sense is not too violent. It is essential however to remember that this train of reasoning is self-infirmative and not self-confirmative.

The only proof of the first proposition that is to be found in Mr Wharton's paper is the *conjecture* that the early Latin alphabet did not contain G or K. But the very fact that K was not used except in a few abbreviations is proof positive that it never dropped out of its alphabetical position, for if so it would never have been restored. And whoever introduced the character G, and however late it was introduced, it certainly was placed in the position occupied by Z, that is, seventh. This is at once an argument in favour of an early date for its introduction, and as near proof as any fact can be, that F and H *never* stood next each other in alphabetical order.

Again the rule that *nm* becomes *rm* in Latin, is, with all due respect to Mr Wharton, not proven. As **nōnima*—*norma* is under discussion, the proof rests on two examples, *carmen* and *germen*. If these are to support **nōnima*—*norma* they must = **canimen*, **genimen* respectively. Then the question arises—Why did *anima* not become **arma*¹? If on the contrary **canmen* **genmen* be the original forms these must have

¹ The answer to this may be that **cānimen* **gēnimen* were the forms; but this would need great hardihood.

been formed or at least been in existence after *nōnima suffered syncope. But *ex hypothesi* *nonima was only *formed* after 200 B.C. and therefore *canmen must have existed later than the time of Plautus. Now as the word occurs twice on the fragments of the XII. Tables which are preserved—and we know how carefully these were cherished—is it likely that a form *canmen if such had existed would have been so completely displaced by the new-fangled *carmen* (in the middle of only the second century B.C.) that Varro should know no better than to derive *carmen* like *Camena*, from *cas-men¹? Again, at least two alternative derivations are possible for each of these words. *Carmen* may (1) come from a root *kar*—cf. Oscan *carneis* Umbr. *karu*²—or (2) it may come regularly from *cas-men* (if Varro's testimony be accepted) either on Mr Conway's hypothesis (*Verner's Law*, p. 14) that accentual laws retained the *r* (*z*), or by supposing the form to have been *cas-i-men and that syncope took place after the change from *s* to *r*-between vowels³. So *germen* may be from *ger*, cf. Skt. *gir* 'verschlingen,' Gk. *βopά* &c.—the sense will be 'edible shoots,' or from the root *ges* of *gero* as above.

The rule is therefore supported by only two examples, both of which are doubtful: if any further consideration is needed to turn the scale against the rule, it may be found in the great improbability of the change on phonetic grounds.

The morphology of *nonima might be called in question as it is a unique example of a Latin adjective formed from a primary numeral by means of the suffix -mo-, but I pass it by, and turn to the question of sense development. That a word meaning simply *ninth* could associate itself mentally with a tool similarly shaped, is to me incredible. Of course much must be left to individual taste and judgment in this matter, as

¹ *L. L.* vii. § 26, p. 70 (Steph.). The passage is corrupt, but I think it cannot be reasonably doubted that this is the sense. Curiously enough he goes on to derive *cano* from this same root *cas*.

² *kelpw* may also belong to this root. *kṛ-iō*—*καίρω*, which becomes *kelpw* as *κταίρω* etc. became *κτείνω* etc. Cf.

s-kṛ-iō—Lit. *skiriu*. The root was therefore *s-ka^r*, and gives also *kṛjo-corium*, and *skṛ-to-scortum*. The guttural may have been velar, see *Etyma Latina*, s. vv.

³ Compare Brugmann's similar explanation of *verna*, *Grds.* ii. p. 137.

one will think easy what to another seems impossible. I do not think however that any of Mr Wharton's parallels bear him out. 'L-square' escapes fully two-thirds of the difficulty. Let Mr Wharton try the effect of 'Gimme twelve' on the next carpenter he meets, and he will appreciate the position of the luckless Roman workman who first (on his hypothesis) saluted his mate with 'cedo normam.'

Mr Wharton says the alphabetical order was more familiar to the ancients than to us—a proposition which will not commend itself to every one; but even if it were true, the knowledge of the alphabetical order of the letters does not necessarily bring with it a knowledge of their *numerical* value. Quintilian could use *sexta* to mean F, but the sense was clear from the context: its proper name was F-*littera* (cf. Cic. *De Div.* i. 13, 23). The parallels from Greek can hardly be taken seriously. The Greek numerical system is of course based on the alphabet¹. What right have we to draw analogies from this to Latin which shows no trace of such a system? Had L *two* numerical values?

A new derivation of *norma* is moreover not wanted, and therefore there is absolutely nothing to be gained by still further loosening the far too lax laws of Latin etymology. It is of course improbable that *norma* is a borrowed word from the Greek *γνώριμη*, for to begin with the *g* would not have been lost unless it was a very early borrowing. Still, *norma* is akin to *γνώριμος*, and is thus derived. There existed in the hypothetical 'Ursprache' a root of the form $\hat{g}(e)n$, and a suffix of the form $\bar{a}$, $\bar{e}$, $\bar{o}$ ², which was affixed to verbal roots in their weak form. We thus get three secondary roots $\hat{g}n\bar{a}$, $gn\bar{e}$, $gn\bar{o}$. The last named form is found in Latin in (*g*)*nosco*. A common adjectival suffix in I.E. was -ro-, which is affixed to this very root in *gnarus*. We are accordingly justified in postulating a pre-Latin $\hat{g}n\bar{o}$ -ro-, secondary adjectival form $\hat{g}n\bar{o}r$ imo-, whence *gnormo*- and *normo*- by regular change. As for the sense, take a plank; there are endless lines to be drawn across it, but only

¹ It is quite probable that the simple numeration from 1 to 24 preceded the more elaborate decimal system.

² These were either one suffix by Ablaut, or three independent suffixes.

one which cuts it square. Why should this not be called *norma linea* 'the line to be known' (or even 'well-known' *pace* Mr Wharton)? If then the perpendicular were called *norma*, the tool used to find it might naturally be so called¹. To some extent confirmatory of this is the fact that the ethical use, which is the earlier, agrees better with the sense 'perpendicular' than with the tool for finding it. This renders it more probable that the name was transferred from the line to the tool than *vice versa*.

The above is not written in any partisan spirit. Mr Wharton's derivation violates no theory of mine, but he has challenged refutation. After weighing to the best of my ability every particle of evidence on both sides I am compelled to believe (1) that *l* never was the ninth letter of the Latin alphabet and (2) certainly not in the form *L*, (3) that *nm* does not become *rm* in Latin, (4) that the change of sense is highly improbable, and (5) that the old derivation is satisfactory.

FROM A NOTICE OF 'A NEW ENGLISH GRAMMAR:
LOGICAL AND HISTORICAL. BY HENRY SWEET,
M.A. PART I.'

(*Athenaeum*, July 23, 1892².)

No one who feels the slightest degree of interest in the nature and history of the English language should fail to read Dr Sweet's instructive work. It will, we hope, revolutionize the treatment, or rather the neglect, which English grammar has generally received, and it should do much towards providing a firm and sound basis for the study. A literary grammar this

¹ So *regula* implies *linea*, afterwards transferred to the instrument.

² [These extracts are republished with the consent of the proprietors of the journal. In its thoroughness and in other points the review is charac-

teristic, and I have therefore ventured to include a few paragraphs which Mr Darbishire himself would, probably, not have deemed important enough to be reprinted. C.]

does not claim to be; as a *logical* grammar it has features of its own; as an historical grammar it is worthy of its author's reputation.

Its chief merits may be briefly indicated. Dr Sweet clearly recognizes that English is not an inflexional language, and he therefore seeks to cut away from his treatment all prepossessions which arise from studying grammar *through* an inflexional language. The result of this is to bring out most clearly and usefully the two great elements of significance in English—position and stress. Dr Sweet's treatment of the latter, indeed, can hardly be too much commended. Another valuable feature is the systematic arrangement by which the several parts of speech are first treated philosophically under the heads of form, function, and meaning, and afterwards historically under Old, Middle, and Modern English; while the pages abound with acute suggestions as to the history and development of individual words, such as the derivation of *sidle* (p. 431), of *-let* (p. 483), and the frequent use of proper names to illustrate phonological doublets. The constant use of phonetic transcription to show the actual pronunciation is of especial advantage in revealing doublets which the ordinary orthography succeeds in disguising—especially, of course, in particles and auxiliaries.

With all these excellences, however, there exist grave faults, and in particular we must confess that the "logical" treatment is not satisfactory. For this, those who know Dr Sweet's paper on 'Words, Logic, and Grammar' (*Trans. Phil. Soc.*, 1875—6) will be in some degree prepared, for when conclusions are reached which overthrow "the whole fabric of Formal Logic," the cold-blooded sceptic begins by doubting the conclusions. Dr Sweet there fell into the error, which even Jevons did not escape, of regarding proper names as connotative, and this he repeats here (pp. 57—59). His proof is that "John" at least implies "male human being"; but this is not so. "John" may equally well imply a horse or a dog. In fact, as Fowler says ('Formal Logic,' p. 21), "it suggests to me these attributes only through the medium of the common terms to which it is referred." Again, the whole question of subject and predicate

seems to us to be misunderstood; see especially p. 46. The constant reference to "logical predicate" is objectionable, as we are nowhere clearly told what it means. If it implies "the predicate as used in logic," all opposition between it and the "grammatical predicate" falls to the ground. If, on the other hand, it means the predicate as that which gives information about the subject, we have a marked contradiction to the statement on p. 18 that "it may sometimes be almost a matter of indifference which idea is regarded as subject and which as predicate." The confusion in the use of the term "subject" is even more serious. It is not only "grammatical" and "logical," but in the treatment of the passive voice (p. 112), as also on p. 77, it appears to be used in the sense of the "subject of the action expressed by the verb," which is a not uncommon, but wholly different sense. Unfortunately, we have a still deeper objection to make: the opposition of "logical" to "grammatical," which occurs not only in the above connexion, but almost *passim*, seems not to have been clearly envisaged. Thus on p. 10 we are told that "every grammatical category is the expression of some general idea—some logical category"; on p. 12 we are introduced to logical categories as substances and attributes, qualifiers, general and special words, &c.; while on p. 19 we find grammatical categories given as words, word-groups, and sentences, followed by derivation, inflexion, and other processes, and proceeding to the parts of speech. But what "logical category" has been given corresponding to a division into words and sentences? Again, on p. 40 it is admitted that logical and grammatical categories may directly contradict each other; and on p. 191 the influence of analogy is besought to reconcile them.

The psychology here and there displayed is, if we may say so, a little amateurish. On p. 12 we learn that "substances are known to us solely by their attributes"; and on p. 13 "that the only way in which we can form an idea of any attribute, such as 'yellow,' is by thinking of a number of yellow substances"; and below, that "it is easier to think of an attribute apart from its substances than it is to think of a *substance apart from its attributes*," while immediately after there is what must

surely be an oversight when the separation of "fire" from the material objects which are consumed is apparently considered a feat of mental abstraction, not warranted by any distinction in fact. There are also a few departures from the strait path of scientific philology which as a rule Dr Sweet follows so closely, as when on p. 21 we are told that "in English, whenever we hear the sound [ng]—as in *king*—we know that it cannot form the beginning of a word"; but in speech *phonetic* words are the only factors, and the ear is not concerned with resolving them; the fact is, therefore, that the ear never *hears* [ng] at the beginning of a word, but no one except a keen observer *knows* that. On p. 178, "It was mainly by the help of metaphor that primitive man was able to enlarge his originally scanty stock of words so as to find an expression for each new idea as it arose in his mind" is perhaps only carelessly expressed; so far from metaphor increasing the stock of words, it is a means of retaining them at par. At p. 189 there is a hidden fallacy in the statement that "the efficiency of a form depends on its phonetic distinctness—a hissing consonant such as *s* being, for instance, preferable to an obscure vowel"; for the fact that we mark difference of form by a hissing consonant may cause us to pay greater attention thereto, and so give it greater "phonetic distinctness," while a people who inflected by vowels would probably develop an acute ear for vowel-distinctions. Nor do we think the explanation of *spoke* as due to *spoken*, from the likeness between "he spoke" and "he has spoken" (p. 190), probable on linguistic principles. More probably the bond of connexion is to be sought in the number of verbs which have imperfect and past participle identical, as, for example, *all* the weak verbs. So also we are inclined to question the explanation of the loss of *hr*, *hl* (p. 262).

As we said above, the historical part is worthy of its author, and hardly admits of any general strictures. The only fault we can find is of omission. Dr Sweet refuses to confine himself to the higher language, but also takes in familiar and even "vulgar" speech. This plan would have been perfection had his knowledge of Northern English been more thorough; but he appears to have personal acquaintance only with familiar

English as spoken south of the Thames, and his treatment is, therefore, correspondingly partial.

The following passages seem to call for comment:—

P. 3. "In Parent Arian past time in verbs was regarded as more emphatic—because more definite—than present time, and so was expressed by reduplication." *A priori* present time seems the more definite, and *a posteriori* it was expressed by reduplication, which definitely past time was not.

P. 55. Is not the distinction between singular and plural nouns, as stated, wholly unnecessary? and is it not drawn from the despised "formal logic"?

P. 59. "Proper names are never arbitrary in their origin"—too strong.

P. 79. "*Who* and *what* also differ from *he*, *she*, and *it* in having a common genitive or possessive form *whose*." Is *whose* (interrogative) ever neuter?

P. 105. The name "preterite future" for "I should see" is bad.

Pp. 159 and 163 (§§ 456 and 472). The classification which separates "I see you are mistaken" from "You are mistaken, I see," is questionable.

P. 174. Why is the answer "I do not know" restricted to "special interrogative sentences"? Dr Sweet is unprepared for the ignorance of some people, who might even give that answer to "Is the moon full to-night?"

P. 187. The doublet *wið* and *wip* has only become dialectal. The latter form is regular in the north of Ireland.

P. 192. "Latin *bibere*...the lip-consonant *b* symbolizes the action of the lips in drinking." May have been true for the primitive speech, but is decidedly doubtful for Latin.

P. 193. "Language thus arose spontaneously in individuals through the habit of associating sounds with ideas, through mimicry, &c. This was done at first merely for amusement: the idea of using these sounds to communicate wishes, information, &c., to others was an after-thought." We fear this would find small favour with Prof. Paul, whose principles Dr Sweet in the main follows [Cf. p. 156 inf. *C.*].

P. 198. "This contraction [nt] never occurs except after certain verbs which are themselves isolated in the contracted form." *Have, may, could, &c.*, are used with this contraction without isolation.

P. 208. "In English this form [the plural *men*] is so dead that even such a noun as *Norman* forms its plural *Normans*." On the other hand, we imagine nine people out of ten say "Mussulmen." The reason is that "Nor-" is felt to be unmeaning; "Mussul-" is dimly supposed to be a "foreign word."

P. 279. We here find the not uncommon, but wholly erroneous idea that vowels can [of themselves] voice a breathed consonant.

P. 282. For the loss of *w* add *answer, boatswain, coxswain*, and the strong *sword*, where the loss must be due to absorption in the *o*-vowel, or more probably the *w* disappeared in sounding the original *e*.

P. 313. We believe that the plural *summonses* has been sanctioned by the "Rules of the Supreme Court," and may be heard any day at the temple of Justice in the Strand.

P. 329. The meaning of *elder* is more pregnant than that of *older*, but surely not "more abstract."

P. 330. *Nigh* is by no means obsolete in Northern dialects.

P. 356 sq. The negative *ought* is ingeniously explained as due to wrong division of "one nought," "seven nought," &c. This is hardly convincing, as such collocations are very unusual. The interchange of positive and negative words is always mysterious; the phrase "Why sholde ye ought?" in the 'Nut-brown Mayde' seems strange on either hypothesis. Just previously, the very common expression "for aught I know" should have been recognized.

P. 358. "In MnE. *either* is now restricted to the alternative meaning." What of

On either side the river lie
Long fields of barley and of rye

in the 'Lady of Shalott'?

P. 360. A trace of the "conjoint" *fif* is kept in *fi'pence*, common in Scotland and the north of Ireland.

P. 385. "The higher language also keeps full *-ed* in many forms where the spoken language contracts, as in *beloved* (bi'ləvid) compared with *loved*." It is obvious that the opposition is between "higher" and "familiar," for both must be "spoken," or the distinction would not come in.

P. 400. *Blent* should not be marked as obsolete when it occurs in the best-known lines of Byron.

P. 403. Another relic of O.E. *scādan*, "separate," is to be found in the north of Ireland¹, where *to shade* means "to part the hair," and *shade*, "the parting," just as *schode* in O.E.

P. 412. "The verb *alight* still keeps the older consonantal inflexion." Surely "Another splendour on his mouth alit" must have occurred to Dr Sweet.

P. 422. Is there absolute proof that the *l* was ever pronounced in *could*? It seems natural otherwise to suppose that it was inserted *after* the *l* in *should* had ceased to be pronounced. In connexion with *dare* on the same page it may be noticed what difficulties most people get into when they try to form a preterite to "I dare say." The connexion is too intimate to allow of "I dared say," and yet one can hardly venture on "I daresaid." The form *durst* does survive, at least in American, but, strangely enough, as a *present*.

P. 440. Again, *rathe* can hardly be called obsolete when the Laureate uses it ('Lancelot and Elaine').

P. 489. *Property* is said to have passed through French changes; does this imply that it comes from *propreté*?

We are growing accustomed to careless, or rather unintelligent, proof-reading from the Clarendon Press. On p. xiii (l. 4 from foot) read § 1067; p. 36, l. 2, *verbs* for "nouns"; p. 154, l. 11 from foot, *yesterday's* for "yesterday"; p. 331, l. 6, (1050) for "(1051)"; p. 364, l. 12, *numerals* (?) for "ordinals"; p. 412, l. 3, *vocalic* for "consonantal"; p. 429, l. 6 from foot, *adjectives* for "adverbs"; p. 494, l. 3, *Pythagorean* for "Pythagorean." Whether the inclusion of *ancient* and *frequent* among "dissyllabic adjectives with the stress on the last syllable," on p. 326 (l. 3 from foot), rests with author or printer

¹ [This was confirmed by Mr Thos. Bayne (*Athenaeum* Aug. 13, 1892) who quoted examples of *shed* = 'parting of the hair' from modern Scotch. C.]

we decline to say; but we feel strongly disposed to hope that "program" belongs to the misprints.

We had intended to add a few remarks on Dr Sweet's favourite "posers" *cannon-ball* and *I had rather*; but this notice is already long enough, although it has been impossible to criticize the logical part exhaustively without reference to the Syntax, which we hope will soon complete this truly admirable work.

THE GÖTTINGEN SCHOOL OF COMPARATIVE PHILOLOGY.

(Incomplete, but published after Mr Darbishire's death in the
Classical Review, Oct. 1893.)

THE school of philology which has Göttingen for its centre, and which is associated with the names of August Fick and Adalbert Bezzenberger, has a deservedly high reputation. It represents a middle stand-point between the conservatism of Curtius and the daring but somewhat arid speculation of Osthoff and Brugmann without falling into the pessimism of Johannes Schmidt. To judge merely by results the school which has given us Fick's Homeric discoveries and Collitz's paper on the palatals, to which may perhaps be added, without prejudging the objects of this paper, Bezzenberger's on the guttural-series, has laid the study under no slight obligation. Within the last two years two works of great weight have issued from this school, and as they are to a great extent interdependent it seems fitting to discuss them both together.

The fame of August Fick may indeed be expected to be greater among succeeding generations than in his own: great as his reputation is, I venture to think it is even yet entirely disproportionate to his merits, and that if he had adopted the

usual advertising methods, he would long ago have been enthroned as the king of philologists.

The fourth edition of his *Vergleichendes Wörterbuch* does not disappoint expectation. More could hardly be said: but if any feature is more worthy of note than the rest, it is the openness of mind which has permitted the discarding in this edition of whatever has become antiquated in the third, however valuable it may have been at the time. That this achievement is not unparalleled the history of philology in England will prove: that it is remarkable is shown by numerous examples to the contrary.

The fact that the work calls itself a dictionary however may lead this encomium to be misunderstood. I do not intend to imply that the latest 'theory' finds a place in it. On the contrary the most impressive fact about the volume is that it presupposes a carefully thought-out system which those who will may formulate from its pages, but is not set out in a lengthy disquisition. The gratitude which all readers of Fick's Homeric works must feel for the mode he has taken of supporting his view (which on the normal lines should have required volumes of 1000 pp. at least) will therefore be increased by this work in which the author's labour is exactly proportional to the reader's benefit.

As I give my adherence to Fick's view in almost every case in which Fick differs from other exponents I wish to state what in my opinion are those differences, and the considerations which support my judgment. On the mass of detail which makes up the body of the work it is not my intention to enter; one word of caution is perhaps necessary. Identifications of the most rash description are to be found on almost every page in apparent violation of all phonetic regularity. It must not be inferred that the author disbelieves in phonetic laws. He is not constructing a water-tight system into which no exceptions can leak: his work is to bring together from different languages words which are *possibly* related. True, the equation violates a law. If the law is right the equation is wrong—but if the equation is right the law is wrong.

The chief points on which Fick's views are distinctive are:

1. The classification of the I.E. peoples.
2. The I.E. system of sounds.

The first of these is a well-known difficulty, and Fick's own championship of the Stammbaum theory against Schmidt's attack will be remembered; in fact to a certain extent the original object of the *Wörterbuch* was to show that successive common epochs could be distinguished by their vocabularies (*Sprachschätze*). Brugmann took an intermediate view—he made eight offshoots from the parent stock, but ascribed to them equal independence. Fick now makes twelve separate descendants which are the representatives of three older groups: instead therefore of an 'Asiatic' and a 'European' unity we now have (1) Asiatic, (2) *centum* and (3) *satem* branches of the main stock, subdividing into (1) Skt., Zend and Scythian; (2) Gk., Latin, Celtic, and Teutonic; and (3) Baltic, Slavic, Thracian, Phrygian, and Armenian respectively. Here I agree with Fick, in his postulate of intermediate unities, but I do not accept his third group. I believe Armenian to belong to the Asiatic branch; and, if Hirt's assignment of the Thracians and Phrygians to the *centum*-people be accepted, Fick's geographical continuity is destroyed.

On the other point the differences are more important.

Fick's classification is as follows:—

[On p. xxix. Fick gives this list of the sounds of the *Ursprache*—

Vowels.

e o ǎ; *ē ō ā*, the latter shortened to *e o a* in 'primary auslaut,' else uniformly to *a*.

Mutes.

<i>k</i>	<i>kh</i>	<i>g</i>	<i>gh</i>	palatalized to
<i>k'</i>	<i>k'h</i>	<i>g'</i>	<i>g'h</i>	before 'bright sounds'
<i>ç</i>		<i>z</i>	<i>zh</i>	
<i>t</i>	<i>th</i>	<i>d</i>	<i>dh</i>	
<i>p</i>	<i>ph</i>		<i>bh</i>	

Liquids.

y and *v* : *i* and *u*
n and *m* : *ŋ* and *m̥*
r and *l* : *r̥* and *l̥*

Spirants.

j, *s* (*j* before soft (*weichen*) sounds) perhaps also capable of forming a syllable (*ſ*).

But on p. xxxvii. he remarks 'nachträglich' that he accepts Bezenberger's three guttural series (*Bezz. Beitr.* xvi. 234), and that therefore this scheme needs modification by splitting the *k*-series into a *k*- and a *q*-series; he does not add whether he considers them to have been both equally liable to palatalization.]

Now, reserving for discussion below in connexion with Bechtel's book¹ the controversial topics of the vowels, sonants and gutturals, it must be pointed out how admirably this system is framed. In the first place the recognition that *e*, *o*, *a* are the *only* vowels, and that *i*, *u* are the 'sonant' forms of *y*, *v*, is strictly logical, and the far-reaching importance of it will be shown immediately². In the second place an equally meritorious feature is the distinction between 'independent' and 'dependent' sounds, which is also novel. It is true the only 'dependent' sound mentioned is *z* [Fick's *f*], but the principle is recognized further by his ignoring *n*, *ŋ* as independent sounds. In point of fact the distinction is historical, not natural, and its neglect is due to the exaggerated respect for the less-developed 'science' of phonetics which has done philology much harm. Phonetically speaking *m* *n* and *ŋ* are exactly similar, but philologically they differ, for Indo-European is only supposed to have possessed the sound *ŋ* because it is *easier* to say *ŋk̂* than *nk̂*. This may be admitted, but it is also easier to say *nt* than *mt* and yet we know that the form for 'hundred' was *kmtom* or *k̂mtom*. Incidentally it is interesting to notice

¹ *Die Hauptprobleme d. idg. Lautlehre*, Göttingen, 1892.

completed, but see 'The Indo-European phonological system' in Part II.

² [This part of the review is not inf. C.]

that Bezzenberger's paper (*Bezz. Beitr.* xvi. 234) proves that the two classes of 'velar' gutturals were 'independent,' while Brugmann's view in the *Grundriss* i. makes them 'dependent.'

Those who know Professor Fick's previous controversial writings will not need to be told that nothing could be more delightful than the style of his introduction or more friendly than its tone towards his opponents and his followers alike. The contrast in this respect on taking up Bechtel is striking and perhaps leads me to do him injustice.

His book is stimulating like Fick's, but less convincing: its professed aim is to give a historical account of the gradual development which has taken place in philological views since Schleicher. He treats in turn the questions *a, e, o*; vowel changes; the gutturals; *l* and *r*. The preface tells us that chapters on the hard aspirates, on the distinction between *z* and *j*, *u* and *v*, and on the original accent were cut out: their loss must be regretted.

The fight that was fought over each of these questions is already in danger of being forgotten, and hence Bechtel's reminder that the dominant views are so young will be no less useful to students than his history of the contest will be interesting to those who passed through it, and the work on the whole has been admirably done, although the style does not rise above the normal German level. Unfortunately, however, he has put in more than history: at first he sums up judicially the result of the evidence, but by degrees this tends to become more and more an exposition of his own views on the subject in question, and these do not always commend themselves.

It cannot be forgotten that Bechtel was one of the most bitter critics of the new philology as represented by Brugmann's *Griech. Gram.* The acerbity of his tone he has greatly modified: it is still the deadly sin not to refer to any previous writer who has thrown out a suggestion in common with your view, but occasionally services are credited even to Brugmann, so that G. Meyer (whose own services by the way are quite neglected) is even deluded into speaking of Bechtel's 'Unparteilichkeit.' If this be not a mistake, German 'Unparteilichkeit' must be, like French calm, relative.

Perhaps the best of Bechtel's expositions are those in which he sums up the evidence against Brugmann's Skt. $\bar{a}$ = Gk. o theory and in favour of I.E. $\bar{o}$ from $\bar{o}y$ + consonant.

Less successful is his treatment of ∂ which he thinks becomes in Greek ι (or υ) as well as a . This, when taken in conjunction with his definition of η as ∂n (which, he does not deny, appears in Greek as a), gives an extraordinary fluidity to one's ideas. A few plausible examples are given for the rule in each case: it would really seem as if even B. requires to be told of the necessity for cogent proof, at all events he differs from the view I have always been taught to consider right, that observation is not as important as right inference. This accounts for his indignation at any neglect of priority as well as for his own looseness of demonstration. I cannot help however adhering to the principle that to establish any rule it is first of all necessary to give some (not necessarily many) strong examples: next to show cause against all recognized derivations which are inconsistent with the rule, and in the third place to define exactly the limits of the rule itself. Some would add a physiological explanation of the phenomenon, but this is I think unnecessary. Once a phonological rule is established by the above method it becomes a fact for phoneticians which they may deal with as they like: the rule itself gains nothing from a physiological description, unless it is otherwise in need of support. It is only the establishment of a rule in this way that gives any claim to possession, and no one who has previously hazarded a guess at the result has any claim to priority because he happens to be right. For example, the first man who said 'German Vater = Lat. pater, German Bruder = Lat. frater' did not anticipate Verner's law, and if any one does ever prove that ∂ becomes ι in Greek, B. has not anticipated him.

The treatment of consonantal questions is reduced by the expulsion of a chapter on the breathed aspirates and another on [γ and v] to a discussion of two *Hauptprobleme* only. One is of course the guttural question in which Bezenberger is on the whole followed, and the other is the l and r question which contains little of note except an attack on Brugmann for not

accepting Fortunatov's law (*B. B.* vi. 215 ff. and *Grundriss* i. p. 211 note). This is a good example of Bechtel's *Unparteilichkeit*: Brugmann says that Fortunatov's law rests to a great extent on uncertain etymologies: this charge Bechtel considers 'ungerecht' because out of Fortunatov's thirty-five etymologies twelve may be defended: to these he adds two of F.'s words but rejects his derivation, and produces again two that F. does not mention. Assuming that Bechtel's approval raises these etymologies above the domain of controversy, it seems still fair to describe Fortunatov's proof as *grössten Theiles unsicher*. As for Brugmann's other charge that *es fehlt nicht an unerklärten Ausnahmen*, Bechtel admits some indeed, but dismisses them with an airy appeal to 'difference of dialect,' which is an explanation taken from the *Grundriss* I., but in its application is as unscientific as much of what has been criticized. Brugmann's position is: Vedic *l* corresponds to a certain field of European *l*; Sanskrit *l* covers a wider field than Vedic *l*, but this wide field of *l* corresponds remarkably to European *l*, therefore it cannot have come through the narrower Vedic field and so represents a different dialect. This is good reasoning, true or untrue, but the exceptions to F.'s law, which B. discusses, are partly *Vedic words*, and therefore by explaining them as due to dialect he is introducing dialectal differences into the Vedic hymns, which is a very different matter and highly improbable on the face of it¹. By way of showing up Brugmann's harsh treatment of F., Bechtel proceeds to imply that he accepts a rule of Weise's in the *Grundriss*, whereas Brugmann merely parenthetically observes that Weise has formulated it. I think a very little study of the *Grundriss* shows that more weight is given to a view which is discussed and refuted than to one which is simply credited to the authority on which it rests.

These remarks are not made with any view to contentiousness, for the only object of attack should be error, but they are necessary for the very reason that the attack is veiled. There is quite enough to criticize in Brugmann's system without attacking him personally or making insinuations of unfairness:

¹ [See Mr Darbishire's own paper on 'The Sanskrit Liquids' in Part II. C.]

and I sum up my view of Bechtel's book by saying that the controversial parts are mostly barren, the original matter negligible, and the historico-critical parts excellent.

What then are the main points of the system which these two books represent? They are three in number. Vocalic formation, sonants, and the guttural system. It must be pointed out to begin with that these three are of very various importance: the last is merely a question of phonology, while the former two are fundamental and radical differences which can hardly be separated in discussion. As is well known, the accepted view places l r η and ŋ approximately on a level with i and u and, as I have said, I would go a step further . . .

[*The MS. ends here, but see 'The Indo-European Phonological System,' inf.*]

PART II. UNPUBLISHED ESSAYS IN THE THEORY OF PHILOLOGY AND IN INDO- EUROPEAN PHONOLOGY

*(in the order in which they were written, so far as it can
be ascertained).*

1. Opening chapters of a (?) Primer of Philology.
2. Shorter fragments on kindred subjects :
 - a.* First lecture of a popular course on Philology.
 - β.* What is Correct Speech?
 - γ.* The Cradle of the Aryans.
3. Principles of Analysis, especially in Semasiology.
4. The relation between Phonetics and Phonology.
5. The Indo-European Phonological System.
6. The Sanskrit Liquids (completed in July, 1893).
7. Miscellanea Etymologica :
 - a.* *σμ-* in Greek.
 - β.* Gr. *ἀλείφω*, Lat. *lībo*.
 - γ.* *τέλσον ἀρούρης*.

1. OPENING CHAPTERS OF A (?) PRIMER OF
PHILOLOGY.

I. DEFINITIONS.

II. ON VARIATION IN LANGUAGE AND THE UNIT-GROUP
OF SPEAKERS.

III. ON THE ORIGIN OF LANGUAGE.

I. DEFINITIONS.

(1) Words are spoken sounds, single or combined, to which, by convention, an idea or ideas is or are attached in the mind of the speaker or hearer.

"*spoken* sounds." Therefore "the word 'dog'," as used in this book, does *not* mean the written letters d o g but the sounds produced by one who says "dog".

"single" as "a" in English¹, "à," "a," "y" in French. Note that "I" in English is not a single *sound*.

"by convention." It is sometimes said that certain words express their own meaning by their sound so that they are intelligible on first hearing. In most cases, as in "Ah!" of pain, the meaning is given by the face or by the sharpness of the note. It is clear then that the idea does not attach to the spoken sound and so such words are excluded from our definition. It also excludes spoken sounds which convey an idea of an object in nature by imitating it or some mark of it, as "Meow" of a cat or "Bow-wow" of a dog. Such imitations may eventually become true words, but as long as they are understood of themselves and not by convention they are simply gestures, and as distinct from true words as prancing about on all fours is from the word "horse".

"an idea." This includes not only names and actions which are the most numerous and easily grasped ideas, but also all

¹ [When not emphasised: it is complex in *I have a book but not THE* simple in *Not a man was there*, but book. C.]

the mental attitudes, processes and states which in combination make up what we call thought. Thus "futurity" is a mental attitude and hence an idea, and the sounds "will" which express it form a *word*. Different tongues differ in the extent to which they analyse mental conditions, and hence a word like *vincam*, "I shall conquer," expresses in itself what requires three words in English. Reject the view that the English *I*, *shall*, and *conquer* express three several parts of the one idea *vincam*, as well as the view that the *-m* of *vincam* is in any way equivalent to the English *I*; still more any idea that it is a word. A word must be capable of being used alone. The answer to *Shall you conquer?* is *I shall*, but the answer to *vincesne?* could never be *-am*. The answer to *Who will conquer?* is *I*, but to *Quis vincet?* is not *-m*.

"speaker or hearer." Thus if I am alone and say "tree," that is a word although no one hears me say it: if I hear a parrot, which I have taught, saying "tree," it is a word although it is attached to no idea in the mind of the parrot.

(2) Language is the communication of ideas from one brain to another by means of words.

"communication." The ideas must be communicated. If I speak to a deaf man there is no language because there is no communication of the ideas, as the sounds do not reach his brain. If I speak to an idiot there is no language because there is no communication of the ideas, as the ideas do not reach his brain.

"brain," not necessarily a human brain. A dog's bark consists of conventional articulate sounds to which both he and his master attach definite ideas: it is therefore a language although a very limited one.

The case of the phonograph or the parrot is the converse to that of the deaf man or the idiot. Here there is communication to one brain, but it is not from another. It is therefore not language "by means of words." Gesture-language or the language of signs is therefore excluded from our use of the term, and, as we have seen, imitative words belong properly to gesture-language.

"words" is to be taken strictly in the sense given by the first definition.

The word "language" has also a concrete sense in which it means an instrument of communication of ideas. In this sense it often appears to mean an aggregate of words, as when we say "Greek is a language," but really it means the special mode in which the Greeks communicated their ideas.

In this sense "language" will be printed to distinguish it from "Language" as defined above in (2).

The word "language" should not be used in the sense of "speech," nor "speech" for "language."

Compare the difference between the abstract and concrete sense of "Writing," as in (1) "Writing is a comparatively modern invention," and (2) "The author left an unfinished writing."

(3) Philology is the science of language.

'science.' Manual employment in a laboratory and pulling things to pieces to see how they work is not the essential meaning of this word. It simply means "knowledge," and the business of philology is to know all about language, just as the business of a botanist is to know all about plants. Now a philologist cannot know much about language unless he pulls words to pieces, and hence people often confuse philology with etymology as if its sole object was to derive words. This is as wise as imagining that the object of a botanist is to pull plants to pieces.

"language." This word must be taken strictly in the sense given by the definition.

On writing.

The word *language* is so regularly applied to written language that the definition given above may seem highly unnatural. However, language existed a very long time before writing was invented and is independent of it. Writing is only a means of rendering permanent and visible what would otherwise pass away in sound: it therefore stands in the same

relation to language that written music does to music played. In fact it was at first used in the same manner. Just as few of us could read off an opera from the score, but would need to reproduce it on some instrument, so the early reader did not read silently as we do but spoke the words aloud and then listened to what he said.

Now it does not in the least matter what pictorial representation of language we use, provided we are agreed exactly as to what the pictures mean. Thus in English we know what sounds are intended by the pictures "house" and "cough," and it is futile to take them to pieces and say that a part of one is "ou" and a part of the other is "ou," and that yet these bits do not represent the same sound. The inconvenience of this arrangement comes in when no direct verbal information is obtainable. Thus if I speak here of "the sound *ou* in English" it has no possible meaning. Or if English became a dead language and had to be learned from books, a student would be likely to pronounce "ou" the same in every word, which would produce very curious English.

As a rule such discrepancies between the spoken sounds and their written equivalents are only possible when the *written* language has had a long existence. When writing is first employed, those who use it will at least endeavour to use the symbols consistently. Accordingly it is much more safe to conclude for Latin that the words were pronounced as they were written than it would be if we were dealing with a modern language; although we know that even in the time of classical Latin discrepancy between writing and speech had begun. In Greek the discrepancies which arose even down to quite late times were few and unimportant: the reason is partly because Greek pronunciation changed more slowly than Latin, and partly because Greek spelling followed the pronunciation more closely.

But there was a time when both Greek and Latin were spoken but not written, and in tracing the history of words it will frequently be necessary to follow them back into that time, and even to the period when we believe the fore-runners of Greeks and Romans alike spoke practically one language.

Now, although the words which they used at these far-off times were never in fact written, it is of course necessary for us to write them if we want to put them in a book; but it is important to be clear when a word is meant which appears in the written records which we possess, and when one is meant which is believed to have been spoken only. Special attention is therefore requested to the following use of letters¹.

1. Spaced type means that the word was spoken only.
2. Greek characters for Gk., Italics for Latin and other languages, mean that the word was so written in that language.
3. If the sound represented by a certain written character in a particular word is specially referred to, then that character is printed in **heavy** type.

Thus "It. *deīcō* Lat. *dīcō*" means the word which was pronounced "deīcō" in Italic times and was written "dīcō" in Latin. (Whether "dīcō" accurately represents the Latin pronunciation or not depends upon circumstances.)

So "Hel. *leīqō* Gk. *λείπω*" means the word which was pronounced "leīqō" in Hellenic times and was written "*λείπω*" in Greek.

Again "I.Eu. *aġō* Gk. *ἄγω* Lat. *agō*" means the word which was pronounced "aġō" in Indo-European times and was written "*ἄγω*," "*agō*" in Greek and in Latin respectively.

And "Lat. *nīvem*" directs special attention to the sound which was represented by *v* in the written word "*nīvem*." "Gk. *λείπω*" directs special attention to the sound which was represented by *ει* in the written word *λείπω*.

II. [ON VARIATION IN LANGUAGE AND THE UNIT-GROUP OF SPEAKERS.]

No two men are precisely similar, and the language of no two men is absolutely the same. This will be clear at once

[¹ This suggestion seems worth recording, but it was not used by Mr Darbishire in his more technical writings and has not therefore been adopted in this book. C.]

from the definition, for although two men who have lived all their lives under very similar conditions will have a very large number of their ideas in common, it is clearly not probable that each will have exactly the same number, neither more nor less; still less that each idea of the one will be exactly equivalent to a corresponding idea of the other; and least of all that each will apply his instrument of speech to expressing his ideas in exactly the same way. Even if this were granted, it would not be sufficient for all the bodily organs which are used in speaking; larynx tongue lips &c. would have also to be precisely similar for both. Therefore it must be affirmed—and it is most important—that no two men have precisely the same language.

Still, just as the fact that no two men are exactly alike does not prevent our classifying them as *men*, so it is also in the case of language. The characteristic of a language is the possession of so many features common to all those who use it as can only coexist with a fundamental unity of structure. This *generally*, but not necessarily, implies that the language possesses a large number of characteristics which are peculiar to itself when we compare it with other languages.

It is found however convenient to proceed from the unit "language" and to make both larger and smaller divisions. Thus certain languages, although now distinct, bear clear traces of having descended at no very remote date from a common source: such languages form a "group." Thus the Romance group can all be traced back to Latin. When the parent-languages of such groups can themselves be regarded as descendants of one still more primeval ancestor, then all the languages of those groups (including their intermediate ancestors) are classed as a "family."

By way of subdivision languages are treated as follows. Within the limits of a single language there may be several districts in each of which the individual languages of all the inhabitants agree much more closely among themselves than with those of their neighbours. Such a district is said to have a *dialect*.

Again, it may be found necessary to subdivide a dialect

into sub-dialects, and there is no limit to such subdivision except that of convenience.

Note that the test of distinction between a dialect and a language is not that of intelligibility. The speaker of one language may be able to make himself understood by that of another, as often happens on the confines of two languages. Or again, the speaker of one dialect of a language may *not* be able to make himself understood by the speaker of another, as if speakers of the broadest Lancashire and the broadest Devonshire were to attempt conversation.

The difference between languages and the difference between dialects is thus like in kind, but different in degree. We know that different languages, Spanish, French and the rest, have sprung from what must have been originally merely dialects of Latin. Political, social, and racial reasons all go to form languages of what were dialects.

When then we have within historic times such a development of languages out of a single language, it is natural to conclude that similar developments may, or indeed must, have occurred in the times of which we have no historic knowledge. It has long been a commonplace that Greek and Latin present resemblances precisely analogous to those of French and Spanish; and hence the inference is drawn that these languages also are developed out of the dialects of some single original.

III. [ON THE ORIGIN OF LANGUAGE.]

If no interchange of thought were possible and every man's knowledge were confined to the direct impressions conveyed from nature to his mind by his own senses, and the inferences he could draw from those impressions, such knowledge would be limited indeed. The bulk of what we know is common property which has been collected by many million sets of senses, communicated from individual to individual, and handed down from generation to generation, increasing in volume by fresh additions at every step. This clearly would have been impossible

without some means of exchanging thought, and that is what we have seen language to be. It is therefore principally if not entirely due to the perfection of this instrument that man has taken his present place in the animal kingdom. It is true that animals possess in a limited degree the power of transmitting impressions, but these can only be of the vaguest and most indefinite kind. We have seen that it is essential in language for an idea to be attached by convention to the spoken sounds. This at once distinguishes it from the mere power of calling up the impression in another being. But how? Surely nothing whatever enters our minds from outside during our whole existence except through the medium of our eyes, our ears, or other sense. All else must take place in the individual mind alone: no direct action of one mind on another is possible. How then is any convention possible? And if the convention be arrived at, how does it effect such an enormous addition to the power of transmitting intelligence? These are very difficult questions and require an effort of imagination before the answer can be sought.

It is impossible to suppose that man existed before language. The developement of brain, which is man's special characteristic, can only have taken place by means of the discovery of some such means of communication. To put it in Darwinian terms, that class of man-like animals, which first varied in the direction of a better system of interchange of sounds, were better able to combine against common foes and therefore were able to become a dominant species: at the same time this very variation was necessarily attended by a marked developement in intellectual capacity, so that it is really true to say that language is not only the cause of the extent of human knowledge, but the cause of our being men at all.

We may imagine then some class of animal (it is unnecessary to speculate on its form), probably living among trees where gestures would be less possible, to have made use of noises in their throat to inform each other of the presence of danger and so forth. Language to this extent we have seen to be possessed by dogs and other animals. Then in some time of great pressure of foreign foes or great dearth of food, only those

which were most quick at catching and understanding the signals and repeating them survived, and so the conversational style of the whole race was improved. Thus A is breakfasting in the jungle and sees a bear. He calls out something which means 'bear,' and something else which means that the bear is coming towards him. Also a third sound which means that he intends to bolt. B hears all this, takes it in, and A and B get off in safety. But C, not having a good ear, thinks A said the bear was to be attacked by all hands, so he rushes valorously up and gets eaten. D has a slow mind and, although he had heard those sounds before when a bear was near, he had never connected the sound 'bear' in his mind with the real bear, and not having *seen* A bolt, he stays where he is and also falls. When it came to combining against scarcity of food, the advantage of the quick minds would be still greater, and all those families who were 'dull in the uptake' would infallibly starve.

From a rough instance like this we can see the beginning of a convention. A was quick enough to make a certain sound for bear, and a certain other sound for wolf. B was quick enough to notice that A made the distinction and to do the same himself. Then A and B *had established a convention*, and each knew which animal the other meant and was able to act accordingly. By degrees all A's and B's kindred would pick up the convention, they would have the advantage over the other families and live them down. In each generation there would follow one or two original spirits who would hit upon fresh "conventions," and thus little by little a vocabulary would be formed. It was a long and great subject of controversy whether the origin of language was imitation of natural sounds or not; and the "Bow-wow" theory, as it is called, has had both adherents and opponents; but from this point of view the whole dispute disappears. The first sounds *may* have been imitative or they may not, or they may have been so in some cases and not in others. Our friend A may have intended to signify a wolf by imitating a wolf's cry, but it was only as soon as the sound is entirely separated from any such association that we have a true word. Or to put it differently. As long as A and B both

signal to each other by imitating the cry of the wolf, so long there is no convention and no language. As soon as B tries to call to A, not what he has heard from the wolf, but what he has heard A utter on previous occasions when a wolf was near, then B is beginning to speak.

Now comes the second question : how does the existence of a convention facilitate intellectual progress? [*The MS. ends here.*]

2. SHORTER FRAGMENTS ON KINDRED SUBJECTS.

a. FIRST LECTURE OF A POPULAR COURSE ON PHILOLOGY.

β. WHAT IS CORRECT SPEECH?

γ. THE CRADLE OF THE ARYANS.

a. WHAT IS PHILOLOGY?

(*The first lecture¹ of a projected course on Philology to a popular audience.*)

Language is the most complex product of human thought. It is one mode, and that the most common, in which thought is expressed. In this it is in the same class as sculpture, music and painting, to which it is also akin in being probably *imitative* in origin. But it is most nearly akin to music as being composed of sound, and therefore transient, unless artificially preserved, as by written signs. This illustration is important because we are prone to regard *literature* as *language*, whereas in truth this is no more the case than if we were to consider a volume of printed music equal to a concert.

Now language may be defined as the communication of ideas by means of speech. Speech therefore differs from language, and it does so in this way. Speech means the physical action which is accompanied by sound and is characteristic of all mankind except mutes, and further means that action wholly irrespective of conveying an idea. Thus if a Chinaman were to address us in his native tongue, we should certainly know that he was *speaking*, but so far from recognizing it as *language*, we should probably say he was talking 'jargon' or 'gibberish'—unless prevented by having attained superior enlightenment. Conversely, when a parrot says "Polly, put

¹ [This lecture contains in another form the substance of the 'Definitions,' p. 150 sup. C.]

the kettle on," sounds strike our ear by which we are accustomed to receive an idea. If she believed them to come from a human being, Polly might answer "Yes, directly," but if she was aware that they came from a bird, they would cease to be language, because they could not *convey* an idea which did not exist. Here again we have speech without language.

Now I should like you to think out what happens in the case of the phonograph for yourselves.

Language then is speech used for the communication of ideas. It therefore implies the existence of at least two human beings (for the use of language by a single individual for the purpose of more clearly following his thoughts, belongs to psychology), and so is dependent upon the organ of speech, the organ of hearing, and the organ of thought. If any one of these is absent no language is possible.

The science which deals with the organ of speech is vocal physiology: with the organ of hearing, aural physiology: with the organ of thought, psychology. Phonetics treats of the production of speech sounds by the organ of speech, and their transmission to the brain by the organ of hearing. It must therefore rest upon vocal and aural physiology, and on acoustics or the science of sound. We are now in a position to see what philology is: it is the science of language, and therefore rests on, but is distinct from, all the sciences just enumerated, exactly as language rests on, but is distinct from, speech, voice, hearing, &c.

Remembering that "brain" has two aspects—mental and physical—we may illustrate the above as follows. (See page 164.)

The objects of Philology.

The object of every science is to discover laws, and accordingly the object of philology is to discover the laws which regulate the transmission of thought by means of language. Accordingly, just as botanists take various flowers to pieces with a view to discovering the laws which govern the growth

certain results are to be obtained. Every new "derivation" means some new comparison and some new light accordingly thrown into the past, but new derivations are only the means by which philology traces the development of languages and are not *objects* in themselves.

The Province of Philology.

Language is constantly changing. This is not to be wondered at when we remember its complex structure. Every one *learns* language through the medium of his ears (we will turn to writing presently). Now let us imagine a child learning to communicate thoughts by language. In the first place the people whom he hears talking possibly do not form *precisely* the sounds they have in their minds: in the second place the child may not hear *exactly* the sounds produced: in the third place he may not gather from them *exactly* the idea they were intended to give: and in the fourth place when he attempts to reproduce them he may fail to produce the sounds he intends. Now any of these aberrations which are serious enough to attract attention will infallibly be corrected in the course of time, but very slight ones will escape notice and be handed on to the next generation which would probably increase them, so that in a few generations the language may have become something that would have been unintelligible to the parents of the child we began with. When we take a language which is learnt partly by eye, as when writing is much used, the causes of change are increased, and the question therefore becomes more complicated; but these changes are themselves the very province of philology. It is therefore advisable to begin with the simpler problem and study the question of changes of *spoken* language first.

Philology applied to Latin and Greek.

It may at first sight seem absurd to say that the best means of eliminating changes due to writing is to begin with dead languages, but the fact is so, although all our records of

dead languages must obviously be *written*. The reason is that in early Greek and Latin we have language as it were photographed at the stage when it was first written down, for at first writing was merely a picture of spoken language, and so none of the changes due to *learning by eye* could possibly have come in. In other words we know that what a Greek wrote down represented exactly what he said as nearly as he could manage it: how far is this from being the case with English! Moreover the effect of writing is even greater on sentence-construction. It is one thing to have a complicated period unfinished on the paper before you, or even to have a mental picture of how it would look when written; and quite another to have to hold all of it in your head when you don't know how to write. For this reason the first written records of any language are valuable as showing the language in a simplicity which it soon ceases to retain. Accordingly the most certain and interesting results of philology have been attained in Latin and Greek, and they are rendered still more valuable by being highly developed inflexional languages, and so greatly in contrast to our own.

β. WHAT IS CORRECT SPEECH?

In recent issues of a foreign scientific publication¹ the question has been discussed: What constitutes correct speech? As most people are ready at a moment's notice to condemn this or that expression as 'wrong' or even as 'not-English,' it may interest them to learn that the learned doctors have found it by no means easy to answer the above question, and even show the proverbial disagreement in their several views.

Of these, four are the most important: 1. The historic, that it is only correct to talk as our ancestors did: this may be called the Tory view. 2. The democratic or Radical, whatever most people say is right. 3. The authoritarian view, that nothing is correct unless sanctioned by a great writer. 4. The Darwinian, that whatever approves itself by fitness is correct. To these our writers now add a fifth, which may be called the utilitarian view: that whatever serves best to convey the speaker's or writer's meaning is correct, and that only.

Now objections at once suggest themselves to each of the above canons of correctness. The "Tory" view is, as usual, utterly illogical, for it involves fixing on some arbitrary epoch up to which the course of change was permissible, nay advantageous, but after which all further change is interdicted. So the "Radical" view labours under its usual fallacy of substituting what is popular for what is right. Yet probably this view, which is at least as old as Horace and gains currency from his well known line, is the most widely held. If it be

¹ [Noreen, *Idg. Forsch.* 1. p. 95. C.]

accepted, language becomes a mere fashion. Besides, there is the difficulty that it is almost impossible to tell what the majority is. It is also irrational that a new form, which must necessarily begin with a minority, should be condemned until its supporters just outnumber its opponents, whereupon it suddenly becomes "correct." Moreover, if the principle of Local Government be applied, it is correct and only correct to say "lydy" in London, while in Devonshire we must say "laädy," and in Scotland "leddy." Then again, highly useful and thoroughly English words which survive in out-of-the-way places—as "limber" in Ulster (compare Shakespeare *Winter's Tale* 1. 2. 47)—are to be condemned because no longer used by the majority. The authoritarian view, like the Tory, has no room for developement, and has no definition of 'a great writer.' Now to our mind the test of a great writer is not only that he uses language correctly, but that he expands it, so that to say correctness is the usage of great writers is equivalent to saying a writer may innovate provided he is justified by success. This view therefore, like the fourth or Darwinian, inasmuch as it leaves the test to the result of time, wholly fails as a criterion for *present* judgment upon speech as correct or incorrect.

As for the view that language is correct in proportion as it is intelligible, two pertinent objections are raised in the very paper referred to. In the first place it involves a continual change of standpoint—it is correct to speak thus to a carpenter, thus to a Cabinet Minister—and in the second it would award the palm to the style of pleading which, to those who are educated up to it, far exceeds ordinary language in precision. Moreover, why should it be less correct for me to speak as is intelligible to myself, than as is intelligible to John Smith my hearer? The rule rests on a false inference from the curious fact that in speaking, as in the other functions of life, we act less as we would than as we imagine is expected of us. Thus most of us modify our language and sometimes our pronunciation to suit our hearers. A curious mental inference sometimes drawn is that what the hearer would *say* must be most intelligible to him. Hence quite sensible adults talk of a "pitty ickle pitchy-book" to children, and sometimes even

attempt to aid a foreigner's understanding by talking bad grammar and bad English.

But are we to resign all hope of a canon because scientists have failed to discover one, or rather have discovered too many? By no means; in this case again lookers-on see the most of the game, and it will occur to any one that the views above given quite fail to draw the most important distinction that exists in language—between spoken and written. The tests of correctness in these two are totally and entirely different. No one would think of talking as he would write, and no writer with any claims to merit writes as he talks. Walt Whitman indeed and Dr Furnivall have tried it, but with what results! Spoken language is a natural product and its laws are the laws of evolution.¹ All changes in it are the result of law either phonetic or psychical, and unconscious in either case. Phonetic change is well known, but the other kind is equally important and may be exemplified by an innovation which is now establishing itself to the dismay of all purists: we mean the misplacement of the adverb in “to regularly attend.” Except to students of their language this change is so firmly rooted that the writer spent some time in vain trying to explain to an able professional man—himself a writer—that there was anything remarkable in it. The psychical law which explains it is that the historic mental connexion, which has been retained ever since the “to” was really inseparable from the infinitive, has lately been superseded by a stronger mental connexion between such phrases as “ought-to” “mean-to” “want-to” and numerous others. Hence we say “I don’t regularly attend but I ought-to,” and so “I know I ought-to regularly attend.”

Here then absolutely no canon of correctness is possible: the whole subject lies in the sphere of evolution, and just as no one can tell what forms of life are destined to survive, so with forms of language.

But written language belongs to the domain of art. [*The MS. ends here.*]

[¹ Cf. “Phonetics and Philology” C, page 5.]

γ. THE CRADLE OF THE ARYANS.

This enterprising race now fills so much of the world's history that it is only natural if wonderment guesses where was its home. Starting from the streets of Calcutta and sweeping to the streets of San Francisco by way of Piccadilly gives a wide area for choice, but no one has yet been bold enough to select any place west of the Rhine or east of the Caspian for the honourable position. Formerly it never was doubted that the slopes of the Urals were the pastures of the infant race, and that successive migrations betook themselves thence to Iran and Hindostan, to Greece, to Italy, to Britain, to Central Europe, and to the shores of the Baltic Sea. But an antagonistic champion has arisen against each of these dogmas, one which admits the migrations, but traces them to a European site, the other denying any common home and asserting that the parents of each group of languages were themselves quite distinct, but merely dwelt in proximity, like beads on a string, round the central table land of Europe. Then one fine day the string broke and the beads got scattered, one rolling as far as India, the others being satisfied with shorter excursions.

The last hypothesis was strongly supported by Dr Taylor in his *Origin of the Aryans*. The chief argument for it is drawn from anthropology, as the measurements of the skulls found in Europe prove that it has been inhabited by the same race since the ice age. It also fits the facts of language tolerably well, for it is undoubtedly true that if Lithuanian, Sanskrit, Greek, Latin, Celtic and Teutonic are arranged in a circle, each language is more like its two neighbours than any other of the

set. Unfortunately the symmetry is broken by Armenian, which does not stand midway between Greek and Sanskrit in character although it does in position.

A clever essay¹ on this subject has just appeared in a foreign periodical, and the writer gives his voice entirely for the single home and places it on the shores of the Baltic. Against the craniologists he has the telling argument that, whereas their limit of time is the ice age, the dispersion limit may only go back two or three thousand years before Christ. His own positive conclusions are entirely drawn from the evidence of language. He shows that the names for the birch, the willow, the pine, the oak, the fir and the beech are common to all the European languages, and many of them have traces in Skt., although in their long wanderings over treeless plains to settle in a tropical climate they had lost the identity of use. The word 'horse' is unknown; farming terms exist, but only the simplest; and, most important of all, he shows good cause for believing that a common term existed for 'sea.' All these facts square with the theory that the 'home' was in the forests on the shores of the Baltic, and with no other. The only hope for anthropologists is to refuse to believe a man who connects *tar* with *tree* and *mare* 'sea' with *brackish*. [*The fragment ends here.*]

¹ Hirt in *Indog. Forschungen*, 1. pp. 464—485.

3. PRINCIPLES OF ANALYSIS, ESPECIALLY IN
SEMASIOLOGY.

[*The following note is prefixed to this untitled but most suggestive fragment ;*

"The letter *h* in Latin, the sounds it represents, and, consequently, the whole subject of the representation of the *mediae aspiratae* in Latin, have engaged my attention for a considerable period, during which I have found the ground to be preliminarily cleared constantly increasing. The following somewhat disconnected studies deal with some of the questions which present themselves for solution."

The second of these 'studies' was to have been, I believe, 'The physiology of the Aspirate,' of which only the title and a few preliminary notes are left. C.]

[PRINCIPLES OF ANALYSIS, ESPECIALLY IN
SEMASIOLOGY.]

The process of tracing back a word to a more primitive state through the sound-changes it has undergone is commonly called by students of language "phonetic analysis." This term is not correctly so used but is more convenient than "phonological analysis." I shall use 'analysis' alone in this sense unless ambiguity would result.

The methods of analysis have in the past few years been

brought to a high degree of perfection, and although much remains to be done in the way of *application*, it may probably be said that the limits of their *scope* have been reached. After all, the number of spoken sounds to be given with certainty to the parent language is limited: not many "roots" contain more than three sounds: and hence we find that the forms, which we are compelled to postulate as primary for widely differing ideas, assume, as we go back, a more and more dangerous similarity of form.

Moreover, the potency of the modern methods is very tempting to the mere dabbler—a criminal from whom philology has suffered more than any other science. When he is given tools with which he can resolve almost any word into one of a few simple elementary forms, *σπαράττει ὥσπερ σκυλάκιον*, and his imagination runs riot with the result.

But consider the state of the case. When the vowel of a root may assume six different forms if not more, when the distinction between velar and palatal gutturals is no longer absolute (Note A¹), when mediae and mediae aspiratae may alternate in a root, it is hardly to be wondered at if surprising results are to be achieved by the aid of any two dictionaries.

The analytical methods claimed and received credit for their rigidity: by elaboration this rigidity has been practically nullified, and we now look for some external standard by which they may be controlled.

There are two departments of the science which may thus be employed: they are morphology and semasiology.

The scientific student already recognizes the importance of the former of these, but it cannot be too absolutely laid down that for scientific purposes the rules of morphology are as rigid as those of phonology. Hence we get our first limiting canon: If the proposed identification, although phonologically accurate, conflicts in the slightest degree with the use in similar formations—whether it be in the form of the root, the formative suffix, or the mode in which they are combined—it cannot be accepted.

¹ [Not written. C.]

The other department which may be looked to for this purpose is even more important, but is unfortunately still on a very insecure footing. It stands on the border line between philology and psychology, and will scarcely give satisfactory results until investigated from both sides. My object here is simply to point out that its processes (if we admit the existence of science) are as certain, and its rules as rigid, as those of morphology and phonology. As matters stand at present a few moderate and cautious minds have the sense to reject derivations which involve too difficult a change of meaning, but it must be admitted that the imagination of most etymologists is too strong to be daunted by any difficulty.

One especially unfortunate consequence follows herefrom: unworthy suspicion is thrown upon identifications which, though at first sight startling, are really possible, and we are thus carried back to phonology [as being the only available test]; which affords another proof, if proof were wanting, how impracticable it is to study independently any branch of the subject.

This result must be emphasized at the cost of repetition. In phonology there are certain rules which a given derivation must follow in order to be admissible. In morphology there are also certain rules with the same effect. Semasiology must give us a third set of rules, which equally should be followed by any true derivation. A derivation may pass any two of these tests and yet be denied if it fail to pass the third. But as the rules of each are formed from the admissible derivations, we see how important it is that the limitations imposed by the third branch should be as clearly defined as those of the other two.

Something has already been done to make the formation of such a code of rules possible, notably by Paul in the fourth chapter of his *Principien der Sprachgeschichte*, and without attempting to exhaust so vast a subject I will mention a few principles which seem to me true, and not sufficiently appreciated by etymologists. I shall classify them under three heads: 1st General principles. 2nd Principles having their origin in physical causes. 3rd Principles having their origin in mental causes. For convenience I shall refer to these as General,

Physical, and Mental¹, but it must be remembered that from the nature of the subject they all—and not the last alone—are psychical in their application.

I. The first general consideration to which attention must be called is the similarity (and the difference) between the mental phenomena to be observed among the *children* of a civilized people and those probably to be assumed for their primitive ancestors. For example on p. 76 f. Paul remarks how the 'common²' meaning which a child attaches to a 'singular' use of a term is frequently either too wide or too narrow. The same holds, with a difference, for a primitive people. In the case of the child and of the primitive man alike the singular use precedes the common and in fact the existence of the latter postulates a fairly high stage of mental development. The stage at which the singular use alone exists may be illustrated by those languages which have a word for each kind of tree but no word for 'tree'—a word for the several tails of different animals but no word for 'tail.' This example is not however to be pressed. What I wish to emphasize is that originally the word 'tail' always meant some particular tail under observation, and only by degrees came to form a general notion.

A peculiarity of the Indo-European language as compared with those just mentioned will be that of forming general notions of very wide denotation with subsequent specialisation by various means. This again implies that in their case perception of similarity was more acute than that of difference (which may perhaps explain the preference shown by all their philosophers (except George Meredith) for the Identical).

The primitive speaker then resembles the child in that he uses his terms rather in a singular than in a common way. He differs from the child in the way pointed out by Paul, viz. that the child has his primitive conception of a 'common'

¹ [The following definition, on a loose sheet, seems to relate to this class. C.]

'Psychical: the derivation implies that the article suggested to the

mind some similar object.'

² I use the logical names 'common' and 'singular' to express what Paul calls 'usuell' and 'occasionell.'

meaning modified and corrected as he hears more and more cases of its application.....

[I have here collected from various memoranda a few 'principles' which appear to have been jotted down quite roughly at different times. C.]

1. Dissimilar meanings which are easily referable to the same personal affection can be identified.

Thus O. C. Sl. *lovŭ* 'chase' = O. Ir. *log* 'pretium': *acquisition* being the root of both.

2. The number of intermediate steps inserted must be carefully tested by 'length of time.' It is more probable that two widely different meanings each needing numerous steps to trace back to a common root will be found e.g. in Celtic and in Skt. than say in Attic and Argive.

3. Meanings must not be traced through particulars but through universals.

4. The primitive meaning is both more particular and more universal, i.e. cognitions are less clear. A simple particular impression is denoted by a root (hence *particular*) and to it are referred the most remotely similar impressions (hence *general*). (Paul, p. 76.)

5. The same language will be generally consistent in its mode of developing meanings and inconsistent chains must not be readily received. It follows that illustrations of similar transition of meaning in another language even though allied are not admissible in proof unless (a) the transition is itself probable; (b) enough language can be adduced to show that the transition was a general one.

Such transitions are :

Space : Time,	
Bright and dull in colour,	$\left. \vphantom{\begin{matrix} \text{Bright and dull} \\ \text{in colour,} \end{matrix}} \right\} : \left\{ \begin{matrix} \text{Shrill and deep or High} \\ \text{and low in sound.} \end{matrix} \right.$

Cf. the phrase 'chromatic scale.'

6. Where a word in any language bears several senses these are to be traced back to a single primitive sense by a

chain *not only plausible in itself* but plausible considered with respect to the general character of the language in question. Where no such plausible sequence can be suggested, the most common explanation will be that two roots of originally different meanings have come to assume the same form.

From the lexicographer's point of view the following corollary may be added: The test whether such a form is to be catalogued as one word or as two depends on the question whether the fusion was so complete that it was felt as one word, or whether the one sense only could be present to the speaker's consciousness at the moment. For example our word *bill* "a tool" and *bill* "an account" are two, but *hang* active and *hang* neuter are now one, the proof being that *hung* is as regularly the preterite of the active as of the neuter¹.

¹ Of course a dictionary is partly historical and therefore would separate these uses: but the present spoken

English contains only *one* verb, and that can be used actively or passively.

4. THE RELATION BETWEEN PHONETICS AND PHILOLOGY

including a discussion of the alleged distinction between 'i and
spirant *y*,' 'u and spirant *w*.'

PHILOLOGY AND PHONETICS.

I do not intend to apologise for using the term philology in the sense of the study of language which it has long borne in English. Objections have been taken to it, mainly because it does not correspond to the German use of "Philologie" but no satisfactory equivalent has been suggested. Jevons indeed declared that the true analogous term was "logology," but it is not to be wondered at that this has found no favour. The German term "Sprachwissenschaft," at all events in its English dress of "Science of Language," is bad because it seems to sanction the narrow view which limits its domain to etymologising. As the true philosopher embraces all wisdom, so the true philologer embraces all language and in its every aspect, and the student of to-day who seeks by comparison and inference to penetrate into the mysterious realms of Indo-European, fights, however humbly, in the same camp as the Bentleys, Porsons, Scaligers, and Lachmanns of the past.

Now with the exception of changes wrought on the surface of the earth, and of actual relics, our whole knowledge of the past doings of mankind is embraced in written language. It therefore follows that philology, at some point or other, touches every science the subject of which is man. This extensive field is all so closely interdependent that it is impossible to treat any portion of it as absolutely distinct, but the broadest line of division is that which opposes language as an expression of thought, to language as the instrument of that expression; the former being more closely related to the mental, the latter to the physical sciences.

Written language as an instrument of expression has a twofold origin. In the first place it may be developed from attempts to represent graphically a mental cognition as in picture-writing. In the second place it may be developed from similar attempts to express not mental cognitions but their spoken equivalents, as in cuneiform writing. All writing is therefore either graphic or conventional.

Graphic writing always tends to become conventional as the sign ceases to represent the concept and comes to stand for the concept's spoken equivalent, and thence for the sound-group apart from any meaning.

The writing of the Indo-European language is all purely conventional.

There is however a third important use of written language which must not be forgotten. At our present state of development written language has come to be very nearly, if not quite, a direct picture of what passes through the brain¹. Originally this was far from being the case: it rather resembled the description of a picture, the picture itself being of course spoken language. So much was this true that for a very long time written symbols did not convey any idea to the brain through the eye alone: but had always first to be translated into their voice-equivalents².

Accordingly we have to deal with this singularly complicated state of things. Man developed speech, and speech grew along its own lines and under its own conditions for an indefinite period. Having reached a certain pitch of development writing was invented and at first faithfully reflected the spoken state. But the conditions of development of written language are very different from those of speech and accordingly written and spoken language soon began to diverge. But to counteract this tendency there always existed the close practical

¹ [Cf. p. 152 sup.]

² This is not only proved etymologically, as by the equivalence of λέγω and *lego*, *reden* and *read*, but illustrated by such fables as that of Acontius and Cydippe. The binding force of Cydippe's vow lay in her *pronouncing* the

words: a modern young lady would have read them at a glance and not fallen into the trap. Of course this goes to prove the antiquity of the legend and not a similar state of things in classical times.

tie between them to obliterate such divergence, and accordingly it is hardly too much to say that each underwent all the developments of the other. Under these circumstances progress was exceedingly rapid and the process of tracing it becomes correspondingly difficult.

As an illustration it may be mentioned that there are at least four kinds of language in common use to-day. 1. Spoken (*a*) natural language, which is used in daily life, (*b*) language of print, as of an orator making a speech which is to all intents read off a book. 2. Written (*a*) as in the "conversations" of a novel, (*b*) as in serious works. And each of these more or less influences all the others: thus the language of an evening newspaper is a compromise between *2a* and *b*.

This then is one of the problems of philology:—to trace the developments of language both written and spoken, and the influence of each in modifying the other. But a simpler problem first presents itself:—to examine how far language was able to proceed independently of writing. This question escapes the complication of the interaction of written and spoken language, but presents of course the difficulty that we depend for our knowledge of language at that time on subsequent written records. But as has been said, written language in its early stages did not appreciably differ from spoken (except in so far as it was a defective representation of the spoken), and therefore the earliest written records of any language afford us a good field for investigating the stage of development which spoken language reached.

The development of spoken language is the outcome of two antagonistic principles—phonetic change and analogy. The former of these is purely physical, the latter purely mental. The tendency of the former is to break down form-groups, which are an important factor in language, the tendency of the latter is to construct them. It is often said that phonetic laws are invariable, and they are contrasted with the erratic workings of analogy, but the contrast is unsound. Every individual has his own particular phonetic-system which may vary from time to time or even from day to day, but his individual peculiarities do not affect language. For this result to happen it is

necessary (1) that the phonetic change should affect the speech of all or at least a very large number of the speakers and (2) that it should not prevent¹ intelligibility. It follows that the enunciation of a phonetic law always tells us what has happened, and therefore it is not a complete statement if it admits of exceptions. For example it will be a phonetic law of Southern English that in the latter half of the 19th century *ei* passed into *ai* and hence that the word "day" was pronounced [as educated people at the beginning of that period pronounced the word] "die." But if by reason of some peculiarity in my ear or vocal organs I pronounce "day" as "dee" and manage to make myself understood, there is nothing to prevent me—my peculiarity will not prevent the rule from being general.

So of analogy. The rules are invariable in exactly the same way. It is true that different minds will form different groups but those which modify language are those which from appropriateness or convenience were formed by a large number of speakers and actually established themselves. Thus I have known speakers confuse the verbs 'ride' and 'row' from the identity of their past tenses. Of course they were at once conscious of the 'mistake' and such an analogy could not exert any lasting or widespread influence. Another example is the formation of a verb "to misle" from the past participle "mised," which is still more curious for three reasons: it is fairly common (I have myself heard two cases and have had two more well attested): the speakers were all unconscious of any error: and it was an analogy entirely of the eye, so that the very agency which in the case of *rode* and *rowed* tends to prevent confusion, tended in this case to create it.

¹ [The MS. has 'should not conflict with,' which I think means 'seriously conflict with,' 'prevent.' It would certainly not be fair to Mr Darbishire

to insist on the broader meaning of the words he used in a draught not finally revised.]

Phonetics is the science of vocal sounds and therefore is linked on the one side to acoustics, on the other to physiology. When it appears, as has been shown above, that the first study of philology is the development of spoken language, and that one factor in that development is phonetic change, it is obvious how essential it is that the aid of phonetics should be called in to prove or disprove every assumed law, and it is now held as an axiom that the study of philology is based on phonetics. I do not think however that it is altogether unnecessary to urge a few limitations which should be borne in mind, noting that by philology I mean for the present Indo-European philology: that group of languages being the only one which has as yet been studied on true philological lines.

1. The province of phonetics is much wider than that of philology.

This is at once obvious when we remember that phonetics includes all possible human voice sounds—philology only those which can be proved to have existed in the given languages. In fact the statement that the science of phonetics studies the possible, philology the actual seems too plain to be worth making. There is however another sense in which the philological field is narrower than the phonetic which is not obvious at first sight. In phonetics as in philology the actual is only limited by the possible, but in phonetics this 'possible' means 'possible for the human organs under any conditions to produce,' in philology it means 'possible for the human organs and at the same time probable with reference to known facts.' While for example it is possible that every distinction known to phonetists existed in Indo-European, it is practically certain that most of them did not, because speech was much less developed then than now, and no one language even at present possesses more than a certain proportion of those distinctions.

The study of the spoken sounds of any language (as distinct from other languages) is called phonology, which also embraces the study of the changes in those sounds during the history of the language. The system of spoken sounds possessed by any language is called its phonological system or more briefly its phonology.

Thus the field of phonetics embraces the phonological systems of English, French, German, &c., of Arabic, of Chinese, and of every other spoken language utterly regardless of classification. Philology studies the phonology of English and of each of the other Teutonic languages and from them deduces a Teutonic phonology on which they all rest. It studies Romance phonology and the remains of Latin literature and builds up an 'Italic' phonology. So with Celtic and each of the other branches of the Indo-European family, and finally, by comparison and reference from these various phonologies, it constructs one common Indo-European phonology to serve as a basis for all of them.

In doing this it must involve the aid of phonetics at every step, for it is continually postulating phonetic changes and it belongs to the phonetist to decide whether such and such a change is or is not likely to have taken place. Or it finds unexplained variations in the changes which certain sounds apparently identical undergo, and may accordingly suppose that these sounds were not really identical; phonetics again from observed facts must [help to] decide.

2. The phonetics appealed to should be constructed from the outside and not from the inside.

A phonetic system constructed on the basis of I.-E. phonology and expressly designed to explain its changes is very likely to mislead, as it will tend to make certain distinctions from the knowledge that those distinctions require to be made. This defect must be observed in Prof. Sievers' treatise. Thus in order to render easy the now proved fact that η became a in Greek and a pure vowel in certain other languages he asserts that η is as much a vowel as a , which is not true. Again he establishes a consonantal form for certain vowels, viz. i and u , but knowing that consonantal i ($\dot{i}$) assumes two different forms when used initially in certain languages he goes on to say that this $\dot{i}$ must be sharply distinguished from the spirant j . And when a philologist then proceeds to explain the different representation of this sound by the phonetic distinction between $\dot{i}$ and j we get a very pretty circular argument. Sievers himself claims that purely phonetic systems are of little use to philology

because they are constructed in ignorance of the problems presented. But for this very reason their testimony is unimpeachable, and it should be an absolute rule not to recognise any distinction for philological purposes which is not recognised by such phoneticians. The converse indeed, as was said above, does not hold [i.e. phoneticians must recognise many distinctions which do not actually occur in (Indo-European) philology]; but, as presumably both our speaking and hearing organs have developed, it is *a priori* improbable that any sound or any distinction that ever once existed has been utterly lost.

Now the distinction between spirant and semi-vowel is not to be found in Sweet.

It is therefore open to us to question the validity of such statements as

1. (Sievers.) "The Ursprache possessed two distinct sounds *j* and *ǰ*."

2. (Brugmann.) "The difference between Gk. ζ initial and spir. asp. initial, both corresponding to Skt. *y*, rests upon the phonetic distinction of *j* and *ǰ* for which see Sievers,"

and it becomes necessary to investigate the facts and examine whether this distinction or another best explains them.

And first as to Sievers' description of the difference between *j* and *ǰ*.

His words are: "Am häufigsten erscheinen als Halbvocale *i* und *u*, weil dieselben an sich wegen ihrer starken Engenbildung geringe Klangfülle haben" "Mit den Spirantischen *j* und *w*, die sich durch stärkere Engenbildung häufig aus den Halbvocalen *ǰ*, *ǡ* entwickelt haben, dürfen diese ja nicht verwechselt werden'."

I confess that to me it is not easy to see what distinction is here indicated, and it is still more difficult to make a practical distinction in utterance. The greater friction of which he speaks might be produced in one of two ways—by increasing

[¹ *Phonetik*³, p. 146. The MS. contains neither quotation nor reference, but I think these words were meant. C.]

the force of the voice-current, and by narrowing the passage. In the former case we simply get a stressed $\dot{i}$. Now almost any consonant may be stressed and yet there is no trace of phonetic variation from that cause in the I.-E. languages. It is therefore against probability that this should be the case here.

On the other hand Sweet (*Handbook*, p. 51 § 164) has already shown that the consonantal nature of an i -vowel is due to its being a high one. The passage is so narrowed that a hiss is heard even if breath and not voice be emitted. If then j differs from $\dot{i}$ in the narrowness of the passage this merely means that j stands to a high i as $\dot{i}$ does to a normal i , which is clearly not what is meant.

A more common statement of the distinction is that while j (better written y) is a consonant pure and simple, $\dot{i}$ is a *glide*. This again is neither very intelligible nor phonetically accurate. To begin with the definition of a glide as given by Sweet (p. 60 § 189) is that it has no fixed configuration but is composed of all the intermediate positions between two other sounds. Now if $\dot{i}$ is the consonantal form of i it must evidently have exactly the same configuration and hence is not truly a glide at all.

This confusion of ideas seems to have arisen from the practice of calling $a\dot{i}$ a diphthong which it properly speaking is not and so regarding the $\dot{i}$ as a glide-vowel.

Again what pronunciation is possible which shall enable an untrained ear to distinguish absolutely and sharply between *tyeg-* where y is a 'consonant' and *tæg-* where $\dot{i}$ is a 'glide'?

A sort of *a posteriori* criterion of the distinction is often given as being that $\dot{i}$ is found in definite functional relations to an i -vowel while y is incapable of such relations. In the absence of any reason why this should be the case, it is of course no explanation, and the statement, however in accordance with facts, does not bear out the distinction between 'consonant' and 'glide'; for, as Sweet (p. 37 § 108) shows, the front open voice consonant which he denotes by j is susceptible of reduction to a vowel. And yet it is no glide.

Lastly there remains the possibility that the distinction meant is that described by Sweet under the name of "buzzing."

It is not very easy to gather what he exactly means by this, for while he defines it in one place as "the audible friction of voice consonants" (p. 31), consonants themselves having been described just above as "the result of audible friction," in other places he appears to make it an accident of consonants as at pp. 37, 38, &c.

On the whole however it seems clear that he does not recognise any two grades of consonants according as they are buzzed or not buzzed; e.g. in § 166, "whenever a voiced unshut consonant is pronounced without a distinct buzz it is to the ear at least a vowel while being held."

Sievers' classification of *a, e, i, o, u, l, r, m, n*, in the same category leads us to expect a similar series of *glides*, '*a, æ, i, ɔ, ʊ, ɛ, ɜ, ɹ, ɻ, ɰ, ɱ, ɯ, ɶ*' and his third series of *spirants* should also be completed, viz. '*a, e, j, o, v, l, r, m, n*.' Of course no such three series exist in nature, and therefore the inference is that either there is no ground for his distinction of three series or his classification of *a, e, i, o, u*, etc., together is erroneous.

Sievers is so far consistent that he does distinguish between spirant and semi-vowel *l* and *r* (*Phonetik*³, p. 104 § 12), but makes no attempt further to describe the former pair of sounds than by saying the distinction is analogous to that between *j* and *i*.

It should moreover be noticed in any theory as to the distinction between these sounds that such distinction is normally made, by any language that makes it, at the beginning of words and there only. Thus Greek shows ζ and ' where other languages show *y* only, ' and ' for *w, v*- and *av*- for *n, μ*- and *aμ*- for *m, λ*- and *aλ*- for *l*, and possibly ρ̇- and *aρ*- for *r*. And Armenian has a similar double representation for each. The fact that—with the exception of the isolated root *tyeq* which will be discussed below—no such distinction has showed itself in the middle of words is surely not without its significance. If Indo-European possessed a 'spirant' *y*, why should it not have occurred in all positions? And why should it not have become ζ in Greek? The old belief that *-aζω* came from *-ajō* would then become perfectly natural.

The foregoing reasons whether sufficient or not have caused

me to feel dissatisfied with the relation between phonetics and philology as inculcated by its latest exponents, and to feel doubts on the system of I.-E. phonology which they have drawn up in accordance therewith. I have no choice then but to attempt to find some better system which agrees with my own views of the extent to which philology depends on phonetics.

I mentioned above the sharp distinction between the development of language *before* and *after* it began to be written, and observed that early written records may be taken to represent tolerably faithfully the contemporary spoken language. There is now another equally sharp distinction to be drawn which is of the very last importance for philology.

Let us picture to ourselves a period when writing was unknown. Language would naturally undergo the usual phonetic changes, an *m*, say, followed by an *s* would probably become *n* and so forth.

Now in such a condition of things it is possible to suppose two sounds existing, formed with different muscular configurations and each representing a modification of two still more different sounds, which to the ear might be practically undistinguishable. What would be the result? The succeeding generation which would learn those sounds by ear alone (and never having had the consciousness of the two prior sounds) would make no distinction. From that time onwards those two sounds are philologically one. It follows that for philological purposes, the phonological system of a language not yet written down must consist solely of sounds which are readily distinguishable by ear. Now let us pass to the introduction of writing. Here the conditions become widely different, for the ear is no longer the sole means of knowledge. We now have three possible categories of sounds. 1. Sounds distinguished in writing (because distinguished in production), but not distinguishable by ear. 2. Sounds distinguished by ear but not distinguished in writing. 3. Sounds in which the evidence of ear and eye agree.

In the first case an example might be imagined as follows: A Greek might produce a vowel intermediate between the *a* and *ε* positions, but conscious that he was intending to produce

a. To the hearer it might seem as if the vowel were so nearly ϵ that he himself would reproduce it ϵ . Now in the time anterior to writing that vowel and *e* would have fused, but when Greek No. 2 sees Greek No. 1 write it *a* and not ϵ , he is conscious that some distinction is intended which he may preserve when reproducing it. Conversely in the second case identity of writing may tend, though very slowly in primitive epochs, to bring about identity of pronunciation. It is quite probable that in the early times of written Sanskrit *a* had three definite values corresponding to primitive *a*, *e*, *o*, but the distinction was ignored in writing as not being strong enough to produce an addition to their already considerable alphabet.

It will be remarked that while instances of the former class are in their nature rare and likely speedily to vanish (for one reason because a speaker when he comes to write down his words judges as much if not more by what he *hears* himself say, as by what he *feels* himself say) instances of the latter class are frequent in all languages, from the extreme poverty of most alphabetical systems. Moreover, until writing plays a large and important part, written identity rarely leads to spoken.

We arrive then at the following principles :

1. A phonological system for an unwritten language must consist of sounds readily distinguishable by ear.

2. The first written expression of that system will preserve all those distinctions so far as is possible with the available means.

5. THE INDO-EUROPEAN PHONOLOGICAL SYSTEM.

This paper was only left in the roughest form, in two drafts, both belonging to an earlier date than the preceding essay, in which Mr Darbshire probably meant to embody them.

THE INDO-EUROPEAN PHONOLOGICAL SYSTEM.

THE broadest distinction which exists among speech-sounds is between those that have duration and those which have not. These again may be further distinguished as those which must have duration, those which cannot have duration, and those which may or may not have it.

Every musical note must have duration : hence in a certain sense all voiced sounds must have duration, but in many voiced sounds the element of voice is subordinate to the other characteristics, which therefore impart their character to the sound. On the other hand certain sounds consist solely of voice : these must necessarily have duration. The test of such sounds is their becoming inaudible when breathed.

The essential element of an explosive is a complete stoppage of the breath or voice current at some point in the passage. These sounds then are incapable of duration. It is true the stop may be held for any length of time ; but this is not duration but cessation. All other sounds are capable of having their duration shortened until they may be classed as momentary, or of having it lengthened to any extent, as if they were pure voice sounds.

The unit of speech is the syllable¹. The second great

¹ [The following paragraphs may be added here from what I think was the earlier draft of this paper. *C.*]

'The characteristic of a syllable in spoken language is that it contains

one sound which bears a tonic accent. There are as many syllables as there are such sounds in the word or words in question and no more. It stands to reason that tonic accent here in-

division of spoken sounds is into those which are capable and those which are incapable of forming syllables.

This classification is related to the preceding one as follows: No momentary sound can form a syllable. Any sound which has duration *may* form a syllable, but does not necessarily do so. This is shown by contraction. Sounds which are necessarily accompanied by duration always form a syllable.

The following is therefore the scheme:

Non-syllable forming	{	Sounds without duration	{	Syllable forming.
		Sounds capable of duration		
		Sounds of duration		

These are the divisions of sounds which are of the highest practical importance for the development of language, and it is therefore greatly to be desired that a terminology existed to express them. This is however not so, and the uncertainty and ambiguity of the existing terms has often been commented upon. The prevailing popular terminology describes 'syllabic' sounds as 'vowels,' non-syllabic as 'consonants.' This is inconvenient as giving no term for those sounds which can assume either function. To remedy this the terms 'consonant: sonant' are sometimes employed to express the difference in syllabic functions, and 'consonant: vowel' to express the difference in form—which is bad as it leaves 'consonant' ambiguous, and further misleads those who still use 'sonant' in the sense of 'voiced.' I propose therefore to confine the terms consonant and sonant to function as syllable-forming elements, to which use they are clearly adapted. For the other classes, the first or momentary class may be called 'stops,' and the third vowels. For the intermediate class I propose to adopt the term

cludes all grades to the very weakest, and does not refer to that *contrast* of accent which causes us to speak of an unaccented syllable.

All sounds which do not in fact bear

such accent are consonants. Better terms for these would therefore be tonics and atonics, but the others are sanctioned by usage'.

'adsonants,' which will reflect the fact that in function they may be either consonants or sonants.

The Indo-European phonological system accordingly assumes the following shape.

Stops	{	Breathed	$q \quad \hat{k} \quad t \quad p : qh \quad \hat{k}h \quad th \quad ph$						}	Consonants	
		Unaspirated	Aspirated								
		Voiced	$g \quad \hat{g} \quad d \quad b : gh \quad \hat{g}h \quad dh \quad bh$								
Adsonants	{	Breathed	s						}	Sonants	
		Voiced	{	(z)	$\hat{l}$	r	n	m			i
			z	$\hat{l}$	r	n	m	i	u	short	
			$\bar{l}$	$\bar{r}$	$\bar{n}$	$\bar{m}$	$\bar{i}$	$\bar{u}$	long		
Vowels	{	Voiced	{	a	e	o	$\varnothing$		short	}	Sonants
			$\bar{a}$	$\bar{e}$	$\bar{o}$			long			

This system includes all the I.-Eu. sounds which have as yet been shown to have an independent existence. There is no reason to add the sounds commonly written $\hat{n}$ and n since they are not independent but appear only before gutturals. I have however included z , although doubtfully, as it has not been shown to exist except as a form of s before voiced consonants. It is therefore probable that z , if its existence may be taken as proved¹, was at least of later date than the other adsonants. Of course this table is not theoretically complete: the list of stops (and possibly of vowels) may have to be enlarged as the facts work out.

This classification is not based on phonetic but on philological grounds, i.e. it does not correspond to or embrace the possibilities of speech, but is simply derived from the facts presented by the study of Indo-European. It does not however conflict in any way with the results of phonetics.

What then are we to conclude as to the² attempt to construct a class of sounds intermediate between explosives and adsonants, the so-called spirants, written y and w (or j and

¹ Thurneysen, *Kuhn's Ztschr.* 30, p. 351.

² [What follows is now somewhat fragmentary. It has been patched together from the two different drafts, and a certain amount of omission was

necessary in order to bring either of them into harmony with the preceding paper, which was written down later. The only alternative was to re-write the whole passage, and this I had no right to do. C.]

v) corresponding to *i* and *u* respectively [v. p. 188 sup.]? The distinction between *y* and *î* is said to be that *y* can never become a sonant. This [v. sup. ib.] is nothing more than an ex post facto distinction, destitute of all rational basis. It entirely leaves out of account the most important fact in connexion with the difference of treatment in Greek and Sanskrit which it professes to explain, viz. that this difference always shows itself when the sounds are initial (Skt. *yasta* : *îsta*-, Greek ' : ζ-). But in Skt. the supposed combination *ay* becomes *e* (*sētē* : *κεῖται*) just like *aî*.

It is therefore much more probable that the distinction was a difference of beginning, the sound usually written *î* being really *î* with the gradual beginning (Sweet's H), and the sound written *y* being *î* with the clear beginning (Sweet's Λ)¹. These differ in that the first is formed by bringing the organs into position for the sound while breath is actually being emitted, the second by not allowing any breath to escape until the organs have taken up their position.

From the explanation of sonant and consonant given above it is easy to see that *Hî* is most likely to pass into the prolonged-sonant *î*, while *Λî* is essentially momentary and so is pre-vaillingly consonantal. This explains why a root of the form *Hîag* would give in its weak form *iġ*, while a root of the form *Λîes* would become *Λîəs*.

If this view is correct it will follow that after initial *t* or *d*—which are formed in much the position of *î*—the consonantal will be more regular than the sonantal *i*, and this is borne out by the fact that the past participle of a root *tîeg* is in Skt. *tyaktús* and in Gk. *σεπτός*². This account moreover forms a natural basis for explaining the difference which Greek preserves. The one sound began with an expiration, which when the consonant was devoiced remained as the spiritus asper: the other was liable, through the tongue's being too much raised, to let a slight *d*- be heard before the *î*, whence ζ by rule.

¹ [Handbook of Phonetics § 195. C.]

tîeg + *təs*, which may perhaps be directly represented by the Skt. form if

² Possibly re-formed, on the model of *σέβομαι*, from **σαπτος* = I.-Eu.

ə became *a* in Skt. after *î*.

Now it is often a useful guiding principle to assume (for purposes of experiment) that sounds of a similar nature underwent analogous treatment, and the discovery of certain facts about one sound should lead us to look out for indications of a similar treatment of its fellows.

Accordingly we should anticipate for initial sounds the following scheme :

$\overset{\cdot}{i}$	$-y$	$\overset{\cdot}{l}$	$-r$	$\overset{\cdot}{m}$	$-n$
$\underset{\cdot}{i}$	$ y$	$\underset{\cdot}{l}$	$ r$	$\underset{\cdot}{m}$	$ n$

and we may hope to find them all discriminated in those languages which discriminate the first pair

In a previous paper (on the Greek Spiritus Asper) I endeavoured to show that such a distinction does exist for $-y$ and $|y$, and that it is plainly preserved in Greek and Armenian, less clearly in Sanskrit. My exposition there was so far wrong that it adopted the prevailing distinction of "spirant" and "glide," which I now believe to be baseless.

[Here the MS. ends. From a few quite rough notes I conclude that Mr Darbishire was at one time inclined to think that some of the phenomena of prothesis in Greek (at least before l , r , and m) were to be explained by this difference, and to assume that I.-Eu. l , r , m with gradual beginning (his $\overset{\cdot}{l}$, $\overset{\cdot}{r}$, $\overset{\cdot}{m}$) became in Greek $\acute{\alpha}l$ -, $\acute{\alpha}r$ -, $\acute{\alpha}m$ - respectively, but as I do not know whether he came to any decision on the point, the reader will understand that it is mentioned here with all reserve. C.]

6. THE SANSKRIT LIQUIDS.

This paper was written out by Mr Darbshire no less than three times in three successive years (1891—3); the last version I know to have been written in the fortnight before his death. The reader may therefore feel confident that Mr Darbshire had finally accepted its principles after full consideration. The Preface, however, is taken from the second of the three versions, as it had not yet been written for the third.

The editorial notes marked with T. are by Mr F. W. Thomas, those with C. by the editor.

THE SANSKRIT LIQUIDS.

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THE SANSKRIT LIQUIDS.

PREFACE.

THE term liquids is conveniently used to denote the sounds *l* and *r*, which, although of very different formation, are closely connected and exercise a curious influence over each other in most Indo-European Languages.

The history of these sounds has not been satisfactorily traced in any of these languages, and accordingly there has been little scope for deducing the primitive state of things. The reason has partly been that Sanskrit—which in general is our mainstay for consonantal sounds—appears to show the wildest confusion in dealing with these particular consonants, while Armenian, which with its full system of liquids should be a good guide, has as yet offered too few words of certain etymology to throw much light behind. It has therefore been the usual practice to assume that Indo-European possessed two liquids *l* and *r*, which were dental in articulation, and two liquid sonants corresponding, $\check{l}$ and $\check{r}$,—that the European languages and Armenian reflect this difference faithfully—and that Sanskrit is quite irregular¹, but with a preponderating number of its *l*'s corresponding to I.-Eu. *l*.

¹ No explanation of this irregularity is forthcoming. Brugmann hazards only one conjecture;—that confusion arose in Indo-European times from dissimilation in the case of redupli-

cated roots (*Grds.* i. § 282). It will be necessary to mention this hypothesis below. The variation in Sanskrit he is compelled simply to record. *Ib.* § 254.

It seemed therefore worth while to attempt to bring some order into the facts presented by Sanskrit, and as any regularity that existed might most naturally be expected in Vedic, my attention is primarily directed thereto. But, as the Vedic remains which have come down to us naturally do not contain a full list of all the words in the language, it seemed useful and interesting to add in each case a few post-Vedic words which obeyed the rule for Vedic.

Having reached this point I am aware that my proper course in accordance with the most approved models would be to plunge at once into lists of examples, but as it is not necessary that an exposition of results should follow the exact order in which those results were arrived at (and as, moreover, such a course is rather a tax on the reader's patience), I intend to begin by narrating what appears to have been the succession of events in the development of the language, with a statement of the effects which they produce in Vedic Sanskrit. Then shall follow a statement of the rules which express those effects, each accompanied by a complete list of examples. All the words in Vedic Sanskrit which contain *l* and do not fall under one of the rules shall next be discussed, and then those words which conflict with the rules in other ways.

CHAPTER I.

NARRATIVE.

WE suppose then that Indo-European had two distinct liquids which in accordance with the received practice may conveniently be denoted by *l* and *r*. There is reason to believe that both were dental in articulation, and that they were adsonants, that is they could be either sonantal or consonantal in function.

It is convenient to adopt the usual notation and distinguish the cases of function as sonants as *ḷ*, *ṛ*—the cases of consonantal function as *l̥*, *r̥*. It is necessary however even at the cost of repetition to emphasize that these two sets, *ḷ*, *ṛ*, as against *l̥*, *r̥*, are phonetically different, and to speak of *l*, *r* as including them both is simply equal to saying that, as sonant and consonant are always interchanging, it is convenient to have some general character for both:—if the signs *l* and *r* disguise the fact that there were always four phonetically distinct sounds, they are misleading.

It is further necessary to repeat that, when *ḷ*, *ṛ* preceded another sonant, a transitional sound was heard which nearly resembled *ḷ*, *ṛ*, but differed in being a glide only. We write accordingly, not *-lḷo-* *-rṛi-*, but *-ḷ'o-* *-ṛ'i-*.

The subject which has now to be considered is: what was the history of these four sounds between the period of Indo-European unity (which as a working hypothesis may be accepted for the present) and the state of the language which

is known as 'classical' Sanskrit. For the sake of clearness a succinct dogmatic account of the conclusions which we believe justified will first be given, and then shall follow the facts on which those conclusions rest.

At the time when the Asiatic contingent of the I.-Eu. family had reached a position which may be conjecturally placed east of the Caspian and north-east of the present Persia, they still retained the system $\bar{l}, \bar{l}', \bar{l}''; \bar{r}, \bar{r}', \bar{r}''$, intact. About this time they divided into two bodies, one of which proceeded south-east and ultimately reached India, while the other kept a southerly and westerly direction, and became the Zend and Armenian branches respectively. (It is also *possible* that the separation of the Armenian branch was prior to that of the other two.)

I. We may distinguish then, as the earliest Sanskrit period, that in which the I.-Eu. system of liquids remained unaltered. There probably remained with this the vowel-triad a, e, o , and the labial affection of the velar gutturals, before all except palatalising vowels.

II. The second period is marked by a transition of the liquid-group from the dental to the lingual position, and this change was uniformly carried out in the case of those which had sonantal function: but for the consonants the change was prevented under certain conditions which will be given immediately. Instead then of

$$\bar{l}, \bar{l}', \bar{l}'' : \bar{r}, \bar{r}', \bar{r}'' ,$$

we get

$$\bar{l}, \bar{l}', \bar{l}'' : \bar{r}, \bar{r}', \bar{r}'' ,$$

where the letters in darker type indicate sounds which have lost their original phonetic value.

This process undoubtedly commenced immediately after the first period. The conditions under which $\bar{l}$ and $\bar{r}$ remained (i.e. retained their dental position) were as follows:—

1. Two liquids in the same word retained each other in the dental position.

(That this statement may possibly be too wide will be explained below.)

2. A labializing velar, a labial explosive or *m*, retained *l̥* in the dental position, when in the same syllable with it¹.

3. A labial explosive immediately preceded by *r̥* causes it to remain dental.

(This is only highly probable ; see p. 238 ff.)

III. In the third period must be placed the origin of the class of *lingual explosives*. This may possibly have begun during the second period, but is logically subsequent to it, as is shown by the constantly increasing number of linguals which are due to the influence of the lingual *r* during the time of which we have actual knowledge. This third period is however of importance as including the genesis of certain linguals which may possibly be the oldest of the class: these arose whenever an *l̥* which had been retained during the last period under the conditions there mentioned happened also to precede immediately a dental consonant: in such a case the *l̥* vanished and the dental was replaced by the corresponding lingual (Fortunatov's Law²).

IV. At the commencement of the fourth period we have the following state of things. (The letters in darker type still have the same meaning; the Indo-European scheme is given first for clearness, and the figures refer to the above rules for the second period.)

Indo-European	$\overbrace{l̥}^{\text{1}}$	$l̥, \bar{l}, l^r$	$\overbrace{r̥}^{\text{2}}$	$r̥, \bar{r}, r^r$
Sanskrit	$\overbrace{l̥, \bar{l}, l^1}^{\text{1}}$	$\overbrace{l̥, \bar{l}, l^1}^{\text{1}}$	$\overbrace{r̥, \bar{r}, r^1}^{\text{2}}$	$r̥, \bar{r}, r^r$
	(1) (2)		(1) (3)	

Now at or before this time, the three pairs of sonants, viz.: *l̥, r̥*; *l̥, r̥*; *l̥, r̥*, had approached each other so closely that the distinction between each pair was no longer of practical importance to the ear. This was moreover the time at which

¹ [By 'in the same syllable' Mr Darbishire means simply that the sounds are at furthest separated by a single sonant, see p. 222 and cf. 220 (as to *nilapṛāṭa*), inf. C.]

² [Bezz. Beitr. 6. 215. C.]

the Rig-veda was first committed to writing¹, and these three pairs of sounds were accordingly represented alike—the first by *r*, the second by *īr* (*ūr*), the third by *ir* (*ur*). Now regard the consonants: it is natural (not necessary) to suppose that as *ḷ* and *ṛ* approached each other, so would *l* and *r*. If then their history had been otherwise identical, we might have expected to find one sign *r* for the consonants, just as we have one sign *r* for the sonants. As a matter of fact however there was a strongly defined distinction—the outcome of different phonetic environment—which opposed *ḷ*, *ṛ* (dentals) to *l*, *r* (linguals). It was this distinction accordingly,—not that between divided and central, which had become unimportant—that was represented in writing, and while the consonantal sign *r*, corresponding to the lingual sonant *r*, was used for the two linguals *l* and *r*, the other sign *l* was reserved for the two dentals *ḷ* and *ṛ*.

The Indian grammarians therefore, who classify *l* as dental and *r* as lingual, are in full accordance with this hypothesis, and all attempts to see in the Vedic *l* and *r* the representatives of Indo-European *ḷ* and *ṛ* have necessarily failed.

But the Vedic poems, even narrowed to the Rig-Veda, are not the composition of a single generation, and there is ample room to distinguish phonological changes even within their limits.

V. Thus we may call the period covered by the Vedic hymns a fifth period. Its characteristics are the breaking up of the above simple liquid system—*l*, *r*, *r* in the above positions—by the influence of analogy. That this was inevitable will at once be seen. By this time the changes in the gutturals had taken place; probably then *k*, *g* &c. had lost their labial affection; at all events there was no clear reason why they should be associated with *l*; and similarly with the labials. On the other hand *r* was the common weak form for both, and

¹ [The date of the earliest Indian writing is a question on which widely different views have been held: cf. Max Müller, *Hist. Sk. Lit.*, pp. 497 sqq., Goldstücker, *Pāṇini*, pp. 13 sqq., We-

ber, *Ind. Lit.* (trans.), pp. 10, 13, &c., Burnell, *South Indian Palaeography*, Schröder, *Indiens Literatur*, pp. 437 sqq., Cust, *Linguistic and Oriental Essays*, i. pp. 27 sqq. and reff. T.]

hence it is not surprising that the relation $al : r$ lost ground in favour of the more frequent, and apparently more natural, $ar : r$. These and similar phenomena will be discussed in greater detail below.

VI. It is convenient to make a sixth period to include later Sanskrit, although this is not usually regarded as the direct descendant of the Vedic dialect, the strongest reason being that l occurs more frequently in classical Sanskrit than it does in Vedic, and that in the majority of these occurrences it corresponds to European l ; whereas if the two liquids had passed through a stage when l and r were fused, it would be unlikely that they should be differentiated in just this way. The question may be left for the present.

CHAPTER II.

REASONS ON WHICH THE ABOVE NARRATIVE RESTS.

THE controversial points in the above summary have now to be examined in detail. As much of the argument is necessarily from facts to theory, and therefore against the chronological order, it is impossible to take the several periods precisely in the order given.

The arrangement will be as follows : First, the reasons for the statements concerning the First Period will be given. Next such of the facts from the Rîg Veda as support the laws laid down for the Second Period, although of course these facts belong to Period IV. Then shall follow an examination of the grounds for placing Fortunatov's Law in the Third Period. Next the irregularities which make their appearance during the R. V. epoch shall be discussed and classified, and finally any conclusions that can be drawn to guide us through the maze of *l* and *r* in the Sixth Period shall receive attention.

§ 1. THE FIRST PERIOD.

Briefly the reasons for taking perhaps a little unusual view of the relation of the Asiatic languages are these. Dr Hirt's paper on *Die Urheimat der Indogermanen*, I. F. I. pp. 464 sqq., seems an almost conclusive proof that the home of the language is to be found, if anywhere, on the shores of the Baltic. Now hitherto the question of the relationship of Armenian to the

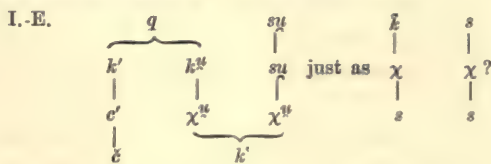
other groups has been a matter of doubt, so far as it has been brought into account at all. But if the original home is to be sought near the Baltic, it is absolutely impossible to suppose that so small and isolated a contingent could have made its way thence, alone, to the position in which we find it. As it is moreover highly improbable that it could have crossed the Caucasus, which even to-day is passable only in two places—it becomes almost necessary to assume that they passed east of the Caspian. The question is of course complicated by our ignorance of the dimensions of that variable basin at that time; but it was probably more extensive than at present, which would render the route between it and the Caucasus (one of the two above referred to) impracticable. If then the Armenians traversed Eastern Europe and passed north and east of the Caspian, the probability is enormous that they did so as part of the Asiatic contingent.

The view which makes Armenian an earlier offshoot from the clan which afterwards subdivided into the Iranian and Indian branches, although above admitted as possible, is probably wrong for these reasons: the geographical position of Armenian and Persian is against it; for the latter lies between Armenian and the probable line of march of the whole body. Unless therefore we assume a prolonged sojourn in Turkestan, after which the Armenians and Iranians broke off at successive intervals to march in the same direction, the theory is untenable. Simpler is it surely to suppose that the body divided into two parts, one continuing its south-easterly course towards India, the other turning more to the west, its most energetic vanguard penetrating as far as Armenia, while the bulk of it settled down in Persia. Again, the *reasons* for the above view are insufficient; they are chiefly the facts that Zend and Sanskrit both agree in obscuring *a*, *e*, *o* to *a*, and agree in replacing *l* by *r*; whereas Armenian in both cases agrees with the languages of Europe. But if this were to guide us, we ought to say that Zend had separated from Sanskrit while Armenian was still joined with it; for Sanskrit has preserved more of the distinction between *e* and *o*, and much more of that between *r* and *l*, than has Zend. On the other hand, there is no reason for surprise

that Zend has gone further than Sanskrit on the same path, if we remember the difference of date at which we begin to know them, and it is no more necessary to assume a unity to account for agreement in such changes as the above than it is to assume a Graeco-Sanskrit unity to account for their agreement in representing q by $\check{a}$. They were kindred by descent; why should not their inherited tendencies be similar? They were both enervated by similar climates; why should not their languages change in the same direction? Lastly, an 'efficient cause' for the view challenged is easily found. Older writers believed Armenian to be a mere Persian dialect: they have been proved to be wrong: therefore there never was an Armeno-Iranian identity!

Another assertion which may perhaps be thought rash is that the velars retained their labial affection at this period. This is not of enough importance one way or another to my subject to need a full defence, but two points may be adduced which render it probable. One is that, as will be shown, the effect of a labialized velar—by which is meant one that becomes a Skt. guttural—is identical with that of a labial in retaining l as dental, and this is naturally to be ascribed to their common characteristic of labialism. This effect is not produced by a velar which becomes a Skt. palatal, whence the inference that at this period q , g , &c. had become k^u , g^u , &c. before a , o , u , vowels, &c., but k' , g' , &c., before e , i , $\bar{a}$ —which after all is not so improbable (Brugmann, *Grds.* I., § 445 *contra*).

The other point which complements this is: Armenian k' represents I.-E. q under certain conditions— $\check{c}$ represents q before palatalising vowels. k' also represents I.-E. su . [Armen. s represents I.-E. k and s .] This points to



The change of the palatal guttural is parallel, but not otherwise important. The point is that the velar guttural has retained

the rounding affection before rounding vowels and lost it before palatalising vowels. After what has been said above it will be clear that such a correspondence need not imply that the two languages suffered this before separation, but since independent arguments show that at a period not very far anterior to the composition of the R. V., Sanskrit (*a*) retained both liquids, (*b*) retained *a*, *e*, *o*, (*c*) retained the labial affection of the velars before rounding vowels, (*d*) lost it before palatal ditto, and that all these characteristics are also found in Armenian, we may assume with confidence that they were characteristics of the common period.

But, it will be said, it is nothing new to suppose that Sanskrit and Armenian can be traced back to a common period, and nothing except the geographical argument has yet been adduced to show that the separation of Iranian and Indian did not take place later, as is generally assumed. This is true; but the first point is to decide what were the common possessions of Sanskrit and Armenian. These we now see to be

1. *a*, *e*, *o* vowels
2. *l*, *r* liquids
3. labial rounding of velars before rounding vowels, probably becoming more and more separate from the explosive preparatory to loss in Skt. and identity with *ʁ* in Arm.
4. loss of labial rounding of velars (or substitution of palatal glide) before *e i* vowels.

It follows that on any assumption these must also have been characteristics of the same period of Zend—if it be allowable to use the three names in speaking of their common existence—and the question is whether the separation when it did take place was into Armenian on the one hand and Indo-Iranian on the other, or into Armenian and Iranian on the one hand, and Indian on the other.

The first of these views was put forward by Hübschmann in 1883, when he published the first part of his *Armenische Studien*. That a brilliant and conclusive demonstration of an original and important view may occasionally be driven to prove too

much is no discredit to the discoverer, and possibly Hübschmann may have modified his views in the light of subsequent investigations. He shows on p. 82 that the Indian and Iranian languages agree in

- (1) *a* for *ā*, *e*, *o*.
 ā for *ā*, *ē*, *ō*.
- (2) *a* for sonant nasal.
- (3) *r* for *r* and *l*.
- (4) *ś* for *s* after *i*, *u*, *k*, *r*:

while Armenian has

- (1) [the vowels distinguished].
- (2) *an*: *a* for sonant nasal.
- (3) *l* *λ* for *l*.
 r *ṛ* *λ* for *r*.
- (4) *s* not changed to *ś* in any certain example.

Now it is a well known principle that mere similar phonetic developments do not prove a common period of history for the languages in which they are found; they may be the outcome of similar tendencies, not of original unity. As regards the obscuring of the original three vowels into *a*, that has also been done by Gothic¹, and therefore is not a sufficiently peculiar change to necessitate unity for those languages in which it happened. So also for the representation of the sonant nasal. On the other hand, if our rules are established, the assumption that Skt. like Zend confused *r* and *l* is disproved. In any case the analogy sought is more graphic than real; for Sanskrit *r* was a lingual; Zend *r* was not.

As then Indian and Iranian had separated *before* the former transferred *r* to the lingual position, and as our rules show that *when* Indian transferred *r* to the lingual position *r* and *l* were both still existing, it follows that the process of assimilating *r* and *l* into one sound belongs to the separate existence of the Iranian branch.

¹ [But v. Brugm. *Gr.* i. § 67, pp. 57—60. T.]

Again, those other processes which are referred to the common life of the Indo-Iranians are equally inconclusive. For example the separation of the velar gutturals into two groups by the following vowel quality (*Grundriss* I. § 445). This has been shown above to have been common to Armenian and therefore belongs to the period of union. Brugmann says "all further changes belong to the separate languages of the (Aryan) family." Naturally, if the fusion of the vowels belonged to their independent stages.

Further, a careful weighing of the points of difference which Hübschmann exhibits between the Armenian and Iranian languages, although more than sufficient to prove the absolute necessity of recognising Armenian as the result of a long independence, does not prove that Armenian and Iranian never formed one group, but rather the reverse.

For it will be noticed that some of the Zend characteristics represent a process which Armenian has carried further, as when the breathed explosives become spirants before consonants in Zend and disappear in Armenian. Then it is only necessary to remember the date at which our acquaintance with Zend and Armenian respectively begins to see that there is ample room for ascribing changes, like the fusion of mediae and med. aspiratae, and others, to their independent existence.

To establish a constructive proof of the converse proposition is not so easy: the test is, which hypothesis yields the best results when worked on. It cannot be said that the Indo-Iranian theory has yielded much fruit as yet. On the other side the following coincidences may be pointed out.

1. I.-E. *sy* becomes *sv* in Sanskrit: Armenian *k'* we have seen to represent $\chi\psi$, Zend shows χw i.e. $\chi\psi$.

2. I.-E. *ty*- becomes *tv*- in Sanskrit: Armenian *k'* (= $\chi\psi$ = $\theta\psi$ = $t\psi$); Zend shows $\mathfrak{S}w$.

The only certain example from Armenian is *k'o* "thy" (Hübschm. no. 293); in *k'ar*- (no. 289) we may have *k'* = *q*, although it is certainly *probable* that *qtur*- has become *tur*-. The change of *t* to the spirant θ has been mentioned above: here the ψ changes it to χ .

3. The palatal $\tilde{k}$ becomes s , the dental sibilant, both in Armenian and in Zend: in Skt. it becomes the palatal sibilant $\ç$. In all probability this change had begun during the common period of all three languages and hence $\tilde{k}$ is distinct from q before e in all three languages. It is probable that $\tilde{k}$ had become χ &c., then Armeno-Iranian developed further together and so both reached s , while Skt. only reached $\ç$ —or $\ç$ may have been reached in the common period¹.

4. I.-E. $s\tilde{k}$ and $\tilde{k}s$ are clearly distinguished in Skt., the former becoming ch (cch) the latter $k\ç$. On the other hand we find in Zend s for $s\tilde{k}$ but $\check{s}$ for $\tilde{k}s$, and in Armenian for both alike $\ç$. That I.-E. $\tilde{k}s$ became Skt. $\check{s}\check{s}$ and thence $k\ç$ (Brugm. *Grds.* I. § 401) is entirely improbable (*dveksi* is probably analogical to the 3d sg. *dvesti* which would be the resulting form of **dyeik-ti* as well as of *dyeis-ti*). That Zend s is only a *pis aller* for $s\tilde{k}$ is shown by the variant $\check{p}$ in the Avesta (*Grds.* I. § 397). Here again in fact we have a clear case of common development in Armeno-Iranian.

$$\begin{array}{lcl} s\tilde{k} & \text{becomes} & s\chi \rightarrow \check{s}\chi \rightarrow \check{s}\check{s} \\ \tilde{k}s & \text{,,} & \chi^s \rightarrow \chi^{\check{s}} \rightarrow \check{s}\check{s}, \end{array}$$

the last stage being carried through only by Armenian, while Zend still conserves a lingering sense of difference.

5. I.-E. qh , $\tilde{k}h$, th , and ph become the breathed aspirates in Sanskrit: in Zend they become the spirants x , $\mathfrak{h}$, f , exactly as do the breathed explosives: in Armenian they vanish, *exactly as do the breathed explosives*, except in one instance *-swal-* = *skhalāmi* (Hübschm. no. 252), where the spirant which was postulated above as preceding disappearance is retained *en évidence* by the s . Therefore we can now assert both that the breathed aspirates developed together in Zd. and Arm., and also that the breathed explosives developed together.

These may serve as samples of what results may be expected from working on the hypothesis of an Armeno-Iranian period,

¹ [Fick, as Mr Darbishire was of course aware, ascribes it to the original Indo-European. *T.*]

which be it remembered is also favoured by the geographical argument. For proving community of development minute correspondences are of far more weight (because less likely to result from broad general laws) than larger points of resemblance; it is less likely, e.g., that two languages should *independently* develop *sy* to *χy*, than that two should independently reduce *a, e, o*, to *a*. And this brings us in conclusion to Hübschmann's fourth distinction between Armenian and Indo-Iranian, that *s* does not become *š* after *i, u, k, r*, in the former and does in the latter, which would be a more or less minute correspondence of the sort. On this there are two remarks to be made—our analysis of *sĕ* and *k̂s* shows that *k̂* did palatalise *s* in Armenian also—and the equations of *p'oši* with Skt. *pām̐ṣu-* and *gišer* with Lat. *heri* (*Trans. Camb. Phil. Soc.* III. 102 [p. 46 *sup.*]) are tolerably satisfactory. It is likely then that this palatalising of *s* began under certain conditions in the common epoch and only became the general law in the separate existences of Zend and Skt. respectively.

§ 2. THE SECOND PERIOD.

In the description of this period it was asserted that

1. Two liquids in the same word retained each other in the dental position.
2. A labialising velar, a labial explosive, or *m* retained *l* in the dental position—when in the same syllable with it.
3. A labial explosive immediately preceded by *r* retained it also in the dental position.

Subsequently it was asserted that in all other cases both liquids became lingual and that in the R̥ig-Veda the signs *r* and *l* were so apportioned that *r* represented the lingual sounds and *l* the dental.

Therefore

Every word in the R̥ig-Veda which shows *l* should come under one of the following heads.

A. It may contain another *l*, in which case *either l* may represent I.-E. *ɾ* or *ʎ*.

B. There may be *k, kh, g, gh* in the same syllable, in which case the *l* = I.-E. *ʎ*.

C. There may be *p, ph, b, bh, m* in the same syllable, in which case the *l* = I.-E. *ʎ*.

D. The *l* may be immediately followed by a labial explosive, in which case it may either = I.-E. *ɾ*, or fall under C.

A. *Rig-Veda* words containing *l*, accompanied by *l*.

The examples are :

<i>alalā</i>	<i>calācala-</i>	<i>lāṅgala-</i>	<i>salalūka-</i>
<i>ulūkhala-</i>	<i>tīlvila-</i>	<i>latāma-</i>	<i>sālila-</i>
<i>kīlāla-</i>	<i>nīlalahita-</i>	<i>çalmali-</i>	[<i>kalmali-</i>]

to which may be added *jaṭhala-, paṭala-, laṣ-*.

Of course the very form of these words brings them in evidence to prove the rule; but it is advisable to add a few comments on the etymology of each.

alalā (in *alalābhavat-*) is simply a cry or ejaculation; but on comparing Greek ἀλαλάζω, ἐλελίζω, ὀλολύζω, and Latin *ululo* it seems probable that some such onomatopoeic reduplication existed in I.-E. Compare also later Skt. *ululu-*.

ulūkhala-: Grassmann's analysis of this word, which means 'a mortar,' into *uru-* 'wide' and *khala-* 'a threshing-floor' can hardly be called convincing. If however this be the true derivation, it is interesting to compare it with *uruloka-*, which should be **ululoka-*. The latter, the correct form, is however not found, either because *uru-loka-* was always mentally identified with the other compounds of *uru-* so as to take the same form (whereas *ulūkhala-* had quite lost all sense-connexion with *uru-*) or because it was a fresh compound of *uru-* and *loka-*.

belonging to a later epoch. The latter explanation is rendered probable by the fact that *uru-loka-* is only found in Bk. x.

kīlāla-. This form would appear to come from a root *qa^xil-*, and a root of exactly this outward appearance will be assumed to exist below (p. 219 *infra*). No sense-connexion with this word, which means 'a sweet drink,' is however possible, while the Greek *κίρνημι* offers an obvious parallel. We may then assume a root *qa^xir-* to account for both and suppose that in Greek the velar lost its labial affection. As for the *-āla* in *kīlāla-*, a word *āla* does exist meaning 'poison': its older sense may have been simply 'drink,' with a development like that of *poison* from *potiōnem*, and of *uenēnum* (see Skutsch *De nominibus Latinis suffixi -no- ope formatis*, diss. 1890). It is, however, contrary to the genius of the I.-E. languages to prefix a root¹ to a word already formed, so as to modify its sense, and we shall see that *āla-* probably meant 'poison' from the first. We therefore analyse *kīlāla-* as *qīr-ē-lo-*,—*ē* being the suffix so commonly found affixed to the weak form of verbal roots.

A few words on *κίρνημι*. The difficulty of the verb-formations *σκίδνημι*, *πίτνημι*, *κίρνημι*, *πίλναμαι* (G. Meyer *Gr. Gr.*² § 497²; Brugmann *Gr. Gr.*² p. 156) is well known. The explanation offered by Moulton (*Class. Rev.* Vol. III. p. 45 *b*—cf. Brugmann *l. c.* p. 236, Per Persson *Zur Wurzelerweiterung* &c. p. 176) that

σκίδνημι belongs to a root *sq[h]ai^td*, *sqhi^td*, whence *scindo* *σχίζω*, but *σκιάννυμι* to a $\sqrt{}$ *sqhed*, and that the other verbs were formed analogically (Per Persson analyses the roots differently, but agrees in principle), is the only account which possesses any plausibility. But *κίρνημι* and *σκίδνημι* may be placed on an equal footing, and this gives a great increase of probability to the explanation. For, in the first place, active forms of *σκιδναμαι* are not found; but only *σκίδνατο*, *σκίδνασθε*, *σκίδνασθαι*, *κίδναι* in Homer, while the regular Homeric parts of *κίρνημι*, *πίτνημι* are *κίρνας* ϵ *κίρνα* *πίτνα* &c. If then we account for *κίρνημι* we can easily explain the less common

¹ [But *kīlāla-* may conceivably stand for **kīlā-āla-*. *T.*]

² [Cf. also §§ 29, 59. *T.*]

but similar forms of *πίτνημι*, just as by accounting for *σκίδναμαι* we explain the less common but similar forms of *πίλναμαι*.

Thus

σκέδασεν (P 649) : *πέλασεν* (Δ 123) = *σκίδναι* (η 130) :
-*πίλναι* (ζ 44)

κέρασσεν (γ 390) : *πέτασαν* (Α 480) = *ἐκίρνα* (η 182) :
πίτνα (Φ 7).

κεράννυμι and *σκεδάννυμι* may be regarded as formed from stems *κερ-ασ-* and *σκεδ-ασ-* respectively. The root of the former is shown to be *ker-* by the *ç* in Skt. *çrīnāti*. The other root *qaiṛ-* whence *κίρνημι* is also found in *καιρός*, in Latin *caerimonia* and probably *sincērus*. The sense of these words shows that the distinction between *κίρνημι* and *μίγνυμι* given by Lid. and Scott s. v. *κράσις*—that *κίρνημι* means chemical, as opposed to mechanical, mixture—is incorrect. The sense present in *κίρνημι* is that of ‘mixing in due proportion’ and hence it suits with the sense of *καιρός* ‘due season,’ *καίριος* ‘seasonable,’ *cairi-monia* ‘due rites’ &c. So also for the sense of *kīlāla-*.

calācala- ‘tottering’ is obviously a reduplicated formation from the root *car-*, of which the I.-E. form was *qeḷ-*. The doubled *l* is therefore as regular as the *r* in *car-*¹.

tilvila- ‘fruitful’ does not at first sight bear any mark of being a true I.-E. word. Perhaps the closest sense-parallel is *taruṇa-* ‘newly sprouted,’ and, if so, the root may be *taṣḷ-*, which Fick *Wörterb.*⁴ I. p. 440 gives for *τηλύγετος ἀταλός*, Lith. *at-toes* &c. In this case it is clear that we have in *tilvila-* the root-form *taḷ-*. Still the formative suffixes remain difficult.

An alternative derivation from the root *teḷ-* of Latin *tellus*, O. Bulg. *tilo*, O. H. G. *dil*, is inferior.

nīlaloḥita-, a Dvandva compound meaning ‘dark blue and red.’ It is of peculiar interest as giving a clue to much of what took place in our *fifth Period*. It may be analysed into two

¹ [earācarā-, however, occurs, cf. Grassm. s.v. Cf. p. 261 inf. T.]

simple adjectives, the first *nīla-* = *nī-ḷo-* from the root *nei* (see below), the second as *rohita-*, the common Vedic derivative of the widespread I.-E. root *reydh-*. The compound therefore represents an original *nīḷoreydhato-* which must have been formed while *ḷ* and *ṛ* were still dental. Then both remained when other liquids became linguals, and we get Vedic *nīlalahita-*. But as no compound is more liable to decomposition than a Dvandva, we also get *nīla-* and *lohita-* used independently. It is true that we find *nīlalahita* only in Bk. x.: but this is probably accidental: *nīlavat* occurs in the earlier books, and also *nīlapr̥sthā-* (which itself would retain the *l* by C *infra*); so *nīla-* must almost certainly have been decomposed and probably from *nīlalahita*—compare the A. V. passage quoted by Grassmann “*nilam asya udaram lohitam pr̥stham*” (xv. 1. 7).

From this it is not an illegitimate conclusion that there may be similar cases of words containing *l* by decomposition, of which no apparent explanation exists, through the loss of the compounds which produced them.

The etymology of *nīla*:—

No very accurate parallel to this word has yet been adduced. It is possible we should recognize a root *nei-* meaning ‘dark,’ of which we have possible extensions in *nei-d-* (cf. Per Persson *op. cit.* p. 35 ff.), whence Gk. *θ-νειδ-ος*, Goth. *naiteins*, Germ. *Neid*—and in *nī-g-* (ibid. p. 15 ff.), whence *nig-ṛó-s*, Latin *niger*. The simple root gives Skt. *nīla-* ‘dark, dark-blue’ for **nīra-* = *nī-ḷo-*, as explained above, also *nīra-* (not R.V.) meaning “(dark-blue) water” from *nī-ḷo-* or *nī-ṛo-*. We have lastly a possible derivative in *veíos* ‘fresh-ploughed land,’ which may easily stand for *veifós* = *nei-ḡó-s*.

* * Of course the ordinary derivation of *veíos* as *nei-ḡo-*, from *vefós* ‘new,’ is phonetically accurate. The only difficulty is the accent, which in derivatives with *-ḡo-* would seem to have been on the root (cf. *návvas*)¹. The dark colour of new-ploughed land is conspicuous: compare

¹ Perhaps this is the reason which makes Fick also postulate *nei-ḡo-*, but he refers to a root *nei* ‘nieder’ (*Etym. Wört.*⁴ p. 500).

τοὶ δὲ στρέψασκον ἀν' ὄγμους
 ἰέμενοι νειοῖο βαθείης τέλσον ἰκέσθαι,
 ἣ δὲ μελαίνειτ' ὀπισθεν.

Σ 546.

lāṅgala- 'a plough' is a word of quite uncertain etymology, and one on which it is scarcely possible even to hazard a guess.

lalāma- fem. *lalāmī* 'with a bright spot on the brow' is a most curious word. It appears to be reduplicated from a root which may either be restored as *la^xm*, in which case cognates appear to be wanting, or as *ra^xm*, and so connected with *ē-ραμαι*¹.

çalmali- 'a kind of tree.' A probable derivation is hardly to be found. Grassmann proposes to compare *çarman*-, which is not impossible. Latin *cēlo*, again, might be referred to, but without much result. It must always be remembered that it is precisely in the names of such natural objects that we are likely to find non-Indo-European words.

salalūka- 'roaming about.' Undoubtedly a derivative of the root *seļ*- 'to move': for the formation Grassmann well compares *jāgarūka*-.

The two suffixes *-rū*- and *-go*- were added to *seļ* in the thematic form, and we get *s|ē|lorūgo*-, which regularly gives *salalūka*-.

salila- 'flowing' comes from *seļ-ə-ļo*-, and accordingly is from the same root *seļ* by means of the suffix *-ļo*-.

It should be noted that the two roots *seṛ*- and *seļ*- (cf. *Trans. Camb. Phil. Soc.* III. p. 92 [p. 35 *sup.*]) are nearly identical in sense, and that by our rule as at present formulated either root would account for these two words last discussed.

The three words *jathala*-, *paṭala*- and *laṣ*- have lost one *l* owing to the changes of Period III. They will therefore be reserved for discussion under Fortunatov's Law.

ADDENDUM. *kalmali*- should have been included, although not R.V., on account of *kalmalīkin*-, which appears to be a

¹ [But cf. *lalāṭa*- 'forehead.' T.]

derivative. The root is *ga^xḷ* (see below on *kalya-*), and of course the second *ḷ* may be either *ḷ* or *ṛ* (suffix *ḷi* or *ṛi*).

The result of this etymological discussion is: three words *alalā-*, *calācala-*, and *tilvila-* probably contain two original *ḷ*s; four more—*ulūkhala-*, *kīlāla-*, *nīlalohita-*, and *salalūka-* are shown to contain one I.-E. *ṛ*, the other being either *ṛ* or *ḷ*: in *salila-* each may be either *ḷ* or *ṛ*, while from the remaining three—*lāṅgala-*, *lalāma-*, *ṣalmali*—no positive result can be attained. There is then no necessity to assume for any word that I.-E. *ṛ + ṛ* becomes Vedic *ḷ + ḷ*, so that our first rule *supra* may be narrowed as follows:

I. "I.-E. *ḷ* in the same word with another liquid (*ṛ* or *ḷ*) retained that liquid in the dental position and was itself so retained."

B. Words in *R.V.* in which *l* is preceded or followed by a guttural in the same syllable.

a. With *l* probably radical.

<i>alakam</i>	<i>kalya-</i>	<i>kulyā-</i>	<i>phaliga-</i>
<i>ālākta-</i>	<i>kāla-</i>	<i>kloṣa-</i>	<i>phalgu-</i>
<i>ulūka-</i>	<i>kitāsa-</i>	<i>khala-</i>	<i>lakṣa-</i>
<i>ulkā-</i>	<i>kilbiṣa-</i>	<i>khalu-</i>	<i>loka-</i>
<i>kalaṣa-</i>	<i>kula-</i>	<i>khilya-</i>	<i>vyalkaṣa-</i>
<i>kalā-</i>	<i>kulāya-</i>	<i>galdā-</i>	<i>vlag-</i>
<i>kali-</i>	<i>kuliṣa-</i>	<i>glā-</i>	<i>ṣulka-</i>
<i>kalp-</i>	<i>kulpha-</i>	<i>-plaka-</i>	<i>ṣloka-</i>

and their compounds or derivatives.

b. With *l* probably or possibly suffixal.

<i>kila-</i>	<i>khargalā-</i>	<i>puṣkala-</i>
<i>kūla-</i>	<i>khela-</i>	<i>-maṅgala-</i>
<i>khṛgala-</i>	<i>daṣaṅgula-</i>	<i>mudgala-</i>

to which may be added *lāṅgala-* (*supra*).

c. The following examples given by Fortunatov of his law show I.-E. *ḷ* retained by the guttural but afterwards vanishing before a dental. How far they are trustworthy will be discussed below p. 242 ff.

<i>kāṇa-</i>	<i>kiṇa-</i>	<i>kuṇḍa-</i>	<i>ghaṭ</i>
<i>kāṇḍa-</i>	<i>kuṭhāra-</i>	<i>khoḍa-</i>	
<i>khaḍ-</i>	<i>kuṇi-</i>	<i>ghaṭa-</i>	

It remains to consider how far *l* in these words can be seen to represent I.-E. *ḷ*.

(a) *alakam* 'in vain.' This probably represents I.-E. *ṇ-ḷoq-o-m* and contains a root *ḷeq* which possibly appears as *leq-s* in *lakṣa-* 'a mark, sign.' Its sense will be 'to cling to' so that *alakam* is literally 'without consequence.' Cognates are not easy to see. Possibly Gk. *λοπός* 'skin, bark' is 'that which clings'; *λεπάς* 'a limpet' from clinging to rocks. It is probable then that the sense of 'skinning' in *λέπω* is secondary, but the derivation is by no means a convincing one. See *infra* under *lakṣa-*.

ālakta- 'smeared with poison' is of interest and importance because like *nīlaloḥita-* it illustrates how irregularities arise. The word *āla-* 'poison' would clearly have no justification for its *l* whatever the derivation, but it happens to occur in R.V. only in this compound, where the *k* at once explains it.

The latter half of the compound of course contains the root *ṇg-* 'to smear,' which appears in Lat. *unguo* &c.

The first element, *āla-*, may contain the suffix *lo-*, in which case the root would hardly be identified. It is better then to analyse *ōḷ-o-* or *ōḷ-ā-* and refer it to the root *oḷ-* 'destroy,' which appears in *ᾔλλυμι* &c. and belongs to the *o* or *ō* scale (Hübschm. *Idg. Vocalsystem* p. 176).

ulūka- 'owl' cf. *çuṣulūka-*.

On comparing Latin *ulula*, O.H.G. *ūwila*, it is clear that the *l* is probably for I.-E. *ḷ*. The root is probably *uḷ-* 'to cry,' and so as applied to the owl will belong to the class of 'descriptive'

names (see *Fox and Wolf* p. 91). Greek *ὀλολύζω* cannot be compared with safety.

ulkā 'radiance' undoubtedly goes with *varcas*, so that *ulkā* = *ulqā*, *varcas* = *velqes*.

See Fick *Wört.*⁴ I. p. 133, who adds *φελχᾶνος* = *Vulcānus*, *ἡλέκτωρ*, *ἡλεκτρον* and *ἄβλαξ*: *λαμπρῶς Κύπριοι*. Of these the last only—the quotation is from Hesychius—is plausible. A much more probable Greek equivalent is however *φέλις* and *φέλομαι*, and these would probably have been long ago compared but for Latin *volup*, which seemed to prove I.-E. *p*. This is still to be found in Per Persson (p. 51); but while the sense of 'pleasure' is naturally developed in an extension of *vel* 'to wish,' it is by no means so easy to reach that of 'hope,' which is more closely akin to that of 'brightness.' Further *vol-up*, and Gk. *καλ-ύπ-τω* probably contain the same suffix, and it is as probable that Latin *volo* comes from *gel* as from *vel*. If then *volup* = *gelup*, its likeness to *φέλις* is nil.

For the suffix *-up* compare also *δρύπτω* beside *δορά*. Per Persson would analyse it into *u + p*, cf. *Wzerw. &c.* p. 160 ff.

These are side-issues: if the relation of *ulkā* and *varcas* is to be affirmed at all, it would be difficult to assign any cause for the variation of *l* and *r* except that of the retention or palatalisation of the guttural, which again depends on the vowel that follows it.

kalaza- 'a vessel, beaker.' This word it is natural to place with *κύλιξ* and so refer it to the root *gel* (*F. and W.* p. 98 *sup.*). It will be formed with the two suffixes *e/o-* and *kō-*, of which the latter is not very common.

kalā 'a small part, a sixteenth.' Grassmann compares *κείρω*; but this is impossible as Skt. *ṣar-* shows. It is highly probable that we have here a root *qol-* in the *o* scale with non-labial velar in the European languages. This is well represented in Greek by *κόλος* and its various derivatives, by *κολιός* 'a woodpecker,' by *κολάπτω* 'to peck' and possibly by *κόλ-αφος*. In Latin we have *cul-ter* and *in-col-umis*, while the strong form *qōl* we find in Gk. *κῶλον* and shall meet immediately in *kāla-*.

Mr Wharton's reference of *incolumis* to *colo* is not so good (*Etym. Lat.* p. 47). No stress can be laid on *columis*, which occurs only in Isidore's glossary.

The sense of "cutting" is then plain in all the derivatives, and may be assigned to the root.

kali-. This is supposed to be a proper name, so it is clearly useless to etymologise.

kalp- 'create, set in order.' The form of this root brings it under no fewer than three of our conditions: the *l* may be retained by the *k*; or by the *p*; or it may represent *ṛ* before *p*. Formally therefore we may be dealing either with *qelṣp-* or with *qerṣp-*, which to begin with excludes Grassmann's citation of Gothic *hilpan* (*kēlṣp-* Lith. *szelpiti* Feist, p. 53).

Two main ideas seem to underlie the numerous senses of *kalp-*; that of 'forming' and that of 'setting to rights.' The latter is prominent in the R.V. *saṅkalpas*, the former in the later use of *-kalpa-* as almost adverbial (Whitney¹ § 1302, 5 p. 449). The two senses are not irreconcilable; but that of "forming" brings in *kṛp-* 'body' 'form' and *kṛpāna-* 'a sword,' which leads naturally to comparing Lith. *kerpu* 'I cut' and so gives an original *qerṣp-*, *qṛp-*.

It may be noted however that the causative *kalpayāmi*, "I set to rights," corresponds so closely in form and sense to Lat. *culpo* (= *colpāxiō*) that possibly a doublet *qelṣp* : *qṣp-* should be assumed to unite with *qerṣp* : *qṛp-* in Skt. *kalp-* : *kṛp-*. Then *culpa* will have been formed from *culpo*: its oldest sense appears to be 'blameworthiness.'

For Latin *corpus* see under *ṣilpa* (*infra* p. 238). The root *qerṣp* could only give **quorpus* and *curpus*, whether from *qorṣp-* or *qṛp-*, while *qerṣp-* is also impossible as *que* does not become *co* in accented syllables before a double consonant (cf. *quercus* with *colo*). This separation of *corpus* and *kṛp-* is another proof that similarity of form and identity of meaning do not always make a good derivation.

The relation *kalp-* : *kṛp-*, which by our rules results from either *qerṣp-* : *qṛp-* or *qelṣp-* : *qṣp-*, is displaced after the influencing causes had ceased (viz. in our Period V.) by the innovation, or rather proportional formation, *kalp-* : *klp-*. Hence the perfect *cāklp-* and causative aorist *cīklpa*, which are only as early as

Bk. X. of the R.V. It was for this root that the rare *l* was introduced (Whitney¹, § 26). The converse levelling by which *r* = *l* is "gunated" to *ar*, we shall find a more common source of irregularity.

kalya- 'healthy.' Greek *καλός* at once suggests itself: if it is to be identified with *kalya*-, we must assume that it had a non-lab. velar and a different suffix; for *καλῖος* would give **καλλος*, while the variation between Hom. *καλός* and Att. *καλός* points unmistakably to *καλρός*. This would stand for I.-E. *k̑l̑ȓo-*, and it would be tempting to analyse *k̑l̑ȓo-* and refer it to the root *k̑leḡ* with a sense-development parallel to that of *κλυτός*—the accentuated -o- suffix commonly forming passive nouns. This would separate *καλός* and *kalya*- completely; the latter must = *qa^xl̑-iō-*.

According to Brugmann (*Grundriss* I. § 439) Slav. *čělu*, Celt. *cěl* and Gothic *hails* are related. If however Skt. *kalya*- is to be referred to this group, it appears that Gothic must show *i*-epenthesis. This is doubtful and so Feist (p. 47, cf. *Grundriss* I. § 641) makes *hails* = I.-E. *k^aailō-*. Now Slav. *čěl* must = *qoīl̑* or *qaiīl̑*-, but Celt. *cěl* can only = *qeīl̑*-, and as *e* and *a* cannot stand in an Ablaut-relation, this proves *qoīl̑* for the Slavonic form. But Gothic *hails* can also = *qoīlō-*, and hence all three point back to the same form. If then epenthesis took place, it did not take place during the Germanic period, but at an earlier epoch. We may then identify *kalya*- by supposing I.-E. $\left. \begin{matrix} qeīl̑ \\ qoīl̑ \end{matrix} \right\} iō-$

to have become $\left. \begin{matrix} qeīlō- \\ qoīlō- \end{matrix} \right\}$ after the separation of the Asiatic branch. For another example of a change being over hastily denied because ascribed to too late a period compare Bechtel, *Hauptprob.* p. 278.

kāla-, 'a defined period of time,' may be identified with Gk. *κᾰλον*, 'a member,' and referred to *qōl̑*- the strong form of *qol̑*-, *supra* p. 224.

Pott's attempted identification with *καίρός* (*K. Z.* IX. 175 *Anm.*) is of course impossible.

kilāsa-, 'spotted.' If the rules given by Brugmann (*Grds.* I. §§ 445 ff.) are a complete statement of the facts, *il* after *k* must necessarily represent *qḷ*'. But as his rules fail to explain *kis*, *kim*, and probably *kila* (*infra* p. 232), and as Gk. *τίλλω*, *τίλαι* and probably Lat. *cilium* are the nearest parallels to *kilāsa*-, but show *i* to be radical, we may be pardoned for doubting the absoluteness of the rule. We may therefore suppose a root **qail*-, *qil* with primary sense 'to scatter.'

If on the other hand *τίλαι* and *cilium* appear too remote, we may find cognates in Sanskrit itself. Cf. *kalana*- 'a spot,' *kāla*- 'black,' and O. B. *kalū* (black) 'dirt.' Other more distant derivatives are given by Fick p. 26. Then the root is *qel*. *kilāsa* however represents the form *qəl* not *qll* (which I prefer to write *qḷ*'). Hence *qəlā*- → *kilāsa* as *é-qḷet* → *akirat*. (This point of principle will be more fully discussed; for the present reference to Bechtel pp. 114 ff. must suffice.)

kilbiṣa- 'guilt,' probably belongs to the same root.

{*kula*- 'family,' 'community'}
{*kulāya*- 'nest'}. These two are obviously

cognate and may be discussed together. The root may be written *quel*, and must be distinguished from *qel* in *carāmi* &c., which gives Gk. *ἐπλετο*, *πόλος*, *κύκλος* &c. (*Grds.* I. § 427 a, b, c, and cf. pp. 97 and 112 *sup.*). The root *qel*- had as its fundamental sense that of 'motion' prevaillingly 'in a circle.' This could not give the sense of *kula*-.

The root *quel* on the other hand has not yet been identified and was likely to escape detection for obvious reasons. The weak form *quḷ* however gives us *kula*- and *kulāya*- directly while the strong *quol* gives *πόλις*, which is akin to *kulāya*- even in suffix. The perseverance of the attempts to identify *πόλις* and *purī*- is only equalled by their want of success. See the discussion in the *Grundriss* I. p. 246 (cf. *Gr. Gr.*² p. 102) and note that an irregular representation of a long sonant liquid is the logical result. As for Vedic *pūr*, it may very possibly be a derivative of the root *peḷ* 'to fill,' and later *purī*- may be a derivative of that; but for Greek and for *πόλις* the sense is quite inappropriate. *quel*- must have here the sense of 'rest

in,' 'dwelling,' which is not the same as 'moving in a circle.' Both roots become combined in Latin *colo* and Gk. *πέλω*, whence from long habit we have some difficulty in seeing the double sense of 'dwelling' and 'circling.' The principle however of formal merging of different roots is a real and important one: it is precisely analogous to syncretism of uses, and must be recognised in many instances to explain sense-transitions otherwise impossible. For changes of sense follow as invariable laws as those of sound, and it will soon be possible to formulate some at least¹.

A hypothesis must be judged by its works. If we assume a root *qul-*, *qul-*, *qul-* meaning 'to dwell,' we explain

1. *kula-*, *kulāya-*.
2. *πόλις* both in form and sense.
3. The wide meaning of *πέλω* and *colo* (*qul-* + *qel-*).
4. *πύλη* and *πύλαια*.
5. *cultus* = *qul-tó-s*, cf. *Grds.* I. pp. 324—5.
6. *πτόλις* as a mistake for *ππολις* on the analogy of *πτόλεμος* [= *pí-ol-* perhaps].

The equation of *kulāya-* with *καλιά* (Bechtel p. 131) is less satisfactory.

* * Fick, *Wörterb.*⁴ I. pp. 26 and 386, has anticipated many of these identifications, but he identifies them on different lines, and admits other impossible relations.

kuliça- 'an axe.' Grassmann's analysis into *ku-liç-* needs modification, as he makes *liç-* = *riç-* = *ερεικ-*, and there would then be no reason for the *l*. Fick (I.⁴ p. 31) derives it from a post-R.V. *kliç* 'to torture,' Slav. *klěšta* 'forceps.' This assumes that *qlik̃-* could become *qlik̃-*, which, in itself doubtful, would, as we have just seen, give **kurīç-* and must be rejected.

Grassmann's analysis *ku-liç-* is obviously the simplest; but instead of *riç-* proving that *-liç-* = *rik̃-*, we now see that *-liç-* proves that *riç-* = *lik̃-*. This then separates *riç-* from *ερείκω*, which has already been done by Brugmann (*Grds.* I. p. 213), who assigns *ερείκω* to Skt. *rikh-*, *likh-*, Lith. *rėkiu*.

¹ [Cf. p. 178 *sup.* C.]

ku-liç-a- and *riç-* therefore contain a root *lik̃-*, the *l* becoming *r* regularly in *riç-* and remaining under the influence of the *k* in *ku-liça-*. This root may also be found in its primitive sense of 'dividing,' 'separating' in Gk. *λίκνον* 'a winnowing basket.' Possibly also Latin *lignum* was originally 'split wood,' not 'collected wood,' and *lictor* was the 'axe-man.'

kulpha- 'a bone,' seems to have no cognates: it can hardly = *κόλαφος*, for which see under *kalā-*.

kulyā- 'a stream.' No cognates seem to have been yet discovered.

kloça- 'cry.' This is formed from the root *ql̃ek̃-*, whence in the form *ql̃ōk̃-* for *ql̃ō(γ)k̃-* (cf. *F. and W.* p. 95 *sup.*) comes Greek *κλώσσω*. Possibly in this and the parallel root *q̃reḡk̃-* (whence *kruç-* and *κρώζω*) a double type existed, viz. *ql̃ek̃g̃-* : *ql̃ek̃k̃-*, cf. *κλώζω* and *Grds.* I. § 469 (7), p. 348.

khala- 'threshing-floor.' Primitive sense possibly 'beaten or pounded hard,' compare *ulūkhala-*. Hence a connexion is possible with Latin *callis* 'a beaten path,' and *callum* 'a hard spot,' although derivations involving hard aspirates are at present always dubious. *χάλιξ* and *χαλεπός* at all events are not connected.

khalu 'really' seems to have no cognates.

khilya- 'barren land,' may be *qh̃el̃-īo-*, and so from the same root as *khala-*.

galdā 'filtering.' Fick *Wörterb.* I.⁴ p. 36 (and Brugmann *Grundriss* I. § 428) has a root *gel-* whence post-Ved. *galati* Gk. *βάλλω* &c. and Germ. *quellen*; hence also Lith. *galas* 'end' and this word. The variations of sense are remarkable, but perhaps not sufficiently so to warrant incredulity. It is noteworthy that *galdā* violates Fortunatov's law, and will be discussed again, p. 248.

glā- 'to be exhausted, downcast.' This is obviously a root of the type discussed by Brugmann *M. U.* I. 1 ff.¹, and goes

¹ [Cf. *Grundriss* II. §§ 578 sqq. pp. 951 sqq. *T.*]

with *yā-*, *jyā-*, *gyā-*, *sā-*, *evā-*, *mlā-*; *πλη-*, *τλη-*, *φρη-*, *θυη-*, *γνη-*, *πλω-*, *γνω-*, *στω-*; *plē-*, *crē-*, *Iā-(nus)*, *(e)-rā-(n)*. It must therefore be analysed *gl-ā-*, but the simple root cannot be found in *jar-* 'to grow old' for two reasons; 1st *jar-* is proved to have had *ġ* by Slav. *zřěti* &c., 2nd it is proved to have had *χ* by *γέρων* &c., whereas *gl-* postulates *gel-*. It is therefore obvious to conclude that this is an 'extension' of the root just mentioned which has that form. Its sense was probably 'to fall' or 'drop,' and from that the metaphorical sense of *gl-ā-* is easily obtained.

-plaka- is only found in *καϑαπλακα-*, and the sense is only to be guessed at. Grassmann compares Lith. *plak-u*, which is formally faultless. It will then belong to the root *plaq-*, *plag-*, Gk. *πλήσσω*, *πληγή*, Gothic *fai-flōk*.

phaliga- is very probably a derivative of *phala* (or its root). See *infra*.

phalgu-. The discussion of this word labours under the 'hard-aspirate' difficulty. It is impossible to suppose any connexion with *φλέγω* &c., *fulgur*, *flamma* &c., which come from *bhleġ-*, while the sense is not appropriate. The sense of *φλέψ* *φλύκτις* &c. (see *F. and W.* p. 98), is appropriate if we suppose that *phalgu-* meant 'trembling' and so 'weak' 'slight': it is applied to *vacas* in the only R. V. passage.

The inversion of the liquid is not infrequent. See Brugmann *Grds.* I. § 259 fin. (and more fully Per Persson p. 97 f.) for examples. His explanation however (if it be so intended) that this depends on the consonantal combination is not satisfactory. Some of the instances favour Dr Fennell's view of the repugnance to 'consonantal weight' shown by accented syllables, but as in many cases both forms are found, e.g. *tatrāptha* : *tatārptha*, it is more probable that the cause lies in the identity of the weak form of root from diphthongs like *ex* &c., and like *xe* &c. Thus *trēp* being reduced to *trp* was wrongly gunated *tarp*, cf. Brugm.'s explanation of *terreo*, *Grds.* I. p. 430, whereas *ib.* II. p. 450 he admits I.-E. metathesis.

¹ [v. 'Indo-European Vowel System,' pp. 3 and 4. T.]

lakṣa- 'a mark, sign' has already been mentioned under *alakam*, where a root *ḷeq-* was postulated, meaning 'to cling or adhere to,' but no very certain cognates were adduced. Perhaps however Latin *lacio* and *laqueus* are reconcilable in form and sense, when they may either cause us to write the root as *ḷaq-*, or have pretonic *a* by Wharton's rule¹.

loka- 'open space.' This word is generally derived from *ruc-* and so = *ḷa^xμqo-*². Thus the *ḷ* is proved, but there also occurs the form *roka-* which will be discussed below.

vyalkaṣa- 'a plant,' bears no trace of I.-E. origin and is exactly the kind of word that would be borrowed. Etymologising is therefore useless.

vlag- 'pressing' cf. *abhivlaṅga-* 'a crowding.' This cannot = Lith. *veržiu*, of which the root is *ver̃g̃h*. It is suggested (*Trans. Camb. Phil. Soc.* III. p. 93 = *sup.* p. 37) that this root in the reduced form *ṽḷg-* may give ὄχλος i.e. *ῥόλχλος*, though without any explanation of the aspirate *χ*. The root however would seem to have a still wider range.

vlag- points to a root of the form *ṽḷa^xg-* (which may be an 'extension' of *ṽḷ-* 'to press'). If we suppose then a double type *ṽḷa^xg* and *ṽḷa^xgh* (*Grundriss* I. § 469 (8) p. 348), we can construct the scheme

I.-E.	Skt.	Gk.	Lat.
<i>ṽḷa^xg</i>	: <i>vlag</i>	—	—
<i>ṽḷa^xng</i>	- <i>vlaṅga-</i>	<i>ῥλαμβ-ανω</i>	[<i>langueo</i>]
<i>ṽḷg</i>	—	<i>ἔ-λλαβε</i> (σ 88 &c.) ³	<i>vulgus</i>
<i>ṽḷgh</i>	—	<i>ὄλβος</i>	
<i>ṽḷa^xgh</i>	—	<i>λάφ-υρα</i>	
<i>ṽḷā^xgh</i>	—	<i>ἐλληφα</i>	
<i>ṽḷgh</i>	—	<i>ὄχλος</i>	

¹ [*Transactions of the Philological Society*, 1888—90, pp. 47 sqq., and *Mém. de la Soc. de Linguistique*, pp. 451 sqq. T.]

² [Cf. Fick, *Wörterb.*⁴ I. pp. 121—2,

304, 540. T.]

³ [The change of *-ῥλ-* to *-λλ-* is not as yet recognised but it may perhaps be justified by that of *-ῥρ-* to *-ρρ-*. G. Meyer, *Gr. Gram.*² § 270. T.]

The Latin *langueo* corresponds phonetically to $\text{ḡla}^{\text{x}}\text{ng}$, but the connexion in sense is not so clear. The Greek derivatives of the word have with the exception of $\delta\chi\lambda\omicron\varsigma$ developed along quite other lines.

ḡlka- 'purchase-money' does not present any cognates.

ḡloka- 'sound, praise &c.' is a derivative of the wide-spread root ḡleḡ [$\kappa\lambda\upsilon\tau\acute{o}\varsigma$, *Ludvig*, *loud*, *Slav*, &c.] by means of the suffix *-qo-*, and so = ḡleḡqo- or ḡloḡqo- . The suffix *-qo-* is therefore the direct cause of the *l* as compared with *r* in *ḡru-*.

(b) Words in which *l* appears to belong to the suffix.

kūla- intensive particle appears to belong with *kis kim* to the relative stem *qi-* with the suffix *-ḡo-*. It does not seem yet to have been explained why the *q* in these words did not palatalise.

kūla- 'a precipice,' is probably from *kū-* 'to look,' i.e. "a place to look out for," and so also contains *-ḡo-*.

The other words cannot be derived with any certainty but all probably contain the same suffix *-ḡo-*. They need not be discussed further now, but are of great importance when we come to speak of irregularities.

(c) These are discussed below.

The result of this etymological examination is as follows. Thirty-two examples were given at the head of this section in which *l* was in the same syllable with an unpalatalised guttural. (By 'the same syllable' is not meant any technical description, but simply that the sounds are separated at furthest by a single sonant.) Of these 32, two—viz. *kalp-* and *phaliga-*—have been referred to other rules; seven—viz. *kali-*, *kulpha-*, *kulyā-*, *khalu*, *phalgu-*, *vyalkaṣa-*, and *ḡlka-*—have presented no plausible etymology. For the remaining 23 etymologies have been suggested which may or may not be accepted, but at least no case for the *l*=I.-E. ṛ can be shown in a single

instance. If different derivatives of the same root as *alakam* and *lakṣa-* are counted as one these 23 are reduced to 17.

We have therefore this evidence to deal with (including *b*). In 40 Vedic words *l* appears in the neighbourhood of *k*, *g* &c. All of those words which can be certainly derived, and 23 which may be plausibly derived, show that this *l* = I.-E. *l̥*. None of the remaining words at all favours the assumption that their *l* = I.-E. *ɾ*.

Therefore if they are I.-E. words, the *l* represents I.-E. *l̥*. Moreover in all of these words *k*, *g*, or *kh* represent I.-E. non-palatalised velars. Beside certain of the words which can be shown to have had *l̥* in the root stand other forms from which the guttural is absent. In those forms the *l* is replaced by *r*.

Therefore the unpalatalised velar is the cause of I.-E. *l̥* becoming *l* in these words.

No more formal demonstration than this is possible. It is interesting to notice that a hint of the truth was given by O. Weise as long ago as 1880 (*B. B.* vi. p. 115), when he called attention to the fact that palatal *k + l* became *çr* in Sanskrit, while Skt. *kr* always was cognate to European *kɾ-*. As this was a very partial view, it is easily dismissed by Bechtel (*op. cit.* p. 389) with the quotation of *çloka-*, one of our strongest examples, and of *çlis-* which is not Vedic, and so does not fall inside our present period.

C. Words in *R.V.* in which *l* is preceded or followed by a labial in the same syllable.

a. With radical *l*.

<i>ulapa-</i>	<i>palasti-</i>	<i>phalgu-</i>	<i>mlā-</i>
<i>ulba-</i>	<i>patāça-</i>	<i>bala-</i>	<i>mluc-</i>
<i>kalp-</i>	<i>palita-</i>	<i>bali-</i>	<i>lip-</i>
<i>kalmali-</i>	<i>plaka-</i>	<i>balbaja-</i>	<i>libujā-</i>
<i>kilbiṣa-</i>	<i>plava-</i>	<i>bila-</i>	<i>lubh-</i>
<i>kulpha-</i>	<i>plāçi-</i>	<i>bilma-</i>	<i>lopāça-</i>
<i>jalpi-</i>	<i>pluṣi-</i>	<i>mala-</i>	<i>çilpa-</i>
<i>talpa-</i>	<i>phala-</i>	<i>mīl-</i>	

b. With suffixal *l*.

<i>apāla-</i>	<i>kapila-</i>	<i>viṣpalā-</i>	<i>ṣimbala-</i>
<i>udumbala-</i>	<i>ṭṛpala-</i>	<i>ṣabala-</i>	<i>su-pippala-</i>
<i>upala-</i>	<i>pipīla-</i>	<i>ṣipāla-</i>	

c. With *l* represented by a lingual by Fortunatov's law. [These words are discussed separately as they involve another element in the problem; see below § 3, p. 241. C.]

The question is then: how far does *l* in these words represent I.-E. *l*, and in how many cases does the etymology point to original *r*?

a. This list must be considerably lightened for discussion: the following examples, *kalp-*, *kalmali-*, *kilbiṣa-*, *kulpha-*, *plaka-*, *phalgu-*, are certainly instances which illustrate the rule; but as the *l* is doubly supported, they have already been mentioned. *ulba-*, *jalpi-*, *talpa-*, *balbaja-*, *ṣilpa-* have a labial explosive immediately following the *l*, and therefore may belong to D, for which they are reserved.

The remaining words are

ulapa- 'underwood.' This occurs only in Bk. x., and is of uncertain formation. Reference to the root *uel* 'to cover' is certainly possible, but no suffix *-po-* is known. Or the root might be *la^xp* and the initial *u* like that in *uloka-*, for which see Bartholomae *B. B.* xvii. 115—16; but for this root again no evidence is found.

? Lith. *lapas* 'a leaf' (which Fick i.⁴ p. 536 takes with *λέπω* adding doubtfully Gothic *laufs*).

palasti- (only in composition as a proper name) is interpreted 'grey.' With *patāṣa-* 'a leaf,' and *palita-* 'grey,' it is probably a derivative of *pala-* 'straw,' which is found not in Vedic but in later Sanskrit. It is cognate with Latin *palea* 'a straw' and possibly with *πάλλω* and *πάλη*.

plava- 'a boat' and *plu-* 'to swim' are from the widespread root *plex-*, *plōx-*, cf. *πλέω*, *πλόος*, &c.

ptāṣi-. The meaning of this word is doubtful. It only occurs in a single passage (in Bk. x.), where it is joined with

yakrt-. From this slight indication Grassmann conjectured that it meant some internal organ, but Fick *Vergl. Wörterb.* i.⁴ p. 85 boldly identifies it with *πραπίδες* with the remark that “ $\pi = q$ entstand in *πραπίδες* durch Wirkung des labialen Anlautes.” By this he apparently means “ $\pi = \phi$,” and is applying a principle which he makes frequent use of in identifications, viz. that a root of the form velar+sonant+palatal or labial+sonant+palatal converts the palatal to velar by assimilation. For instances cf. his work at pp. xxviii., 19, 30, 78, 90, 91, 93, 94. Such an effect is not intrinsically impossible, but as he uses it, it is quite unscientific. The assimilation in question must have been either I.-E. or a feature of an individual language or both. If it were I.-E., we could not get Skt. *plāci-* corresponding to Greek *πραπίδες*; and the form **bhérçō* (p. 91) must have become **bhergo*. If on the other hand it belonged to the separate languages, why do we find it now explaining Skt. *bhargas* (p. 92), now Lith. *peku* (p. 78), *bligsti* (p. 94), and now Gk. *πραπίδες*, although each language has many words which show no such tendency?

It will appear from a subsequent section that the sphere of *l* has already begun to widen in R. V. x., and therefore the *r* does not make the derivation impossible, but this, combined with the doubt as to the sense, and the abnormal assimilation, are perhaps sufficient to warrant its rejection.

pluṣi- ‘an insect.’ The closest parallel appears to be Arm. *low* ‘a flea,’ Hübschmann, *Arm. Stud.* i. no. 126, who compares it with Gk. *ψύλλα*, Lat. *pūlex*, Slav. *blŭcha*, Lith. *blusa*. The Sanskrit word is however closer than any of these, as *low* points to **plūsos*. The Greek and Latin words again can only be identified with each other by supposing *s-pusl-a* to give *ψύλλα* and *pusl-ek-* *pūlex*. Comparison with *pluṣi-* therefore fails, while in the Balto-Slavic words the *b* is strange for *p* before *l*.

The Teutonic word, our ‘flea,’ is found in almost every branch of that group. Kluge connects it with *fliehen* and a root *tleṭyk- tḷuk-*, which appears to be a modification of Osthoff’s *tlek-* for Gothic *plahsjan* and Lat. *locusta*, *P. and B. B.* XIII. 412 f.

Feist’s observation (*Got. Etym.* p. 122) is certainly to the

point here: "*Fliehen* und *Springen* sind...sehr verschiedene Thätigkeiten, die nur den Begriff der Bewegung mit einander gemein haben."

Prof. Skeat derives the word from a root $\sqrt{\text{PLU}}$ 'to fly or jump.' Without going so far as to find a cognate in Skt. *plu-*, we may perhaps conjecture that the Teutonic word did contain *plu-* and so is more or less cognate to *low* and *pluši*.

phala- 'a fruit.' Grassmann compares *phāla-* 'a ploughshare,' and makes the sense 'bursting' with root *phal-*. Others compare Gk. δ -*φελος* (e.g. Hoffmann in *B. B.* XVIII. p. 155). The breathed aspirate makes any identification difficult. Greek *φάλος* &c. cannot be compared, as Lith. *balu* shows that *bh* was here original. On the whole the best cognates are Skt. *sphaṭ-*, *sphuṭ-*, *paṭ-*; *ψαλῖς*; Lith. *spāliai*; Germ. *spalten*, cf. Gk. *ἀσφάλαιξ*, *σπάλαξ*, which are given by Per Persson (see *op. cit.* p. 33 &c.).

<i>bala-</i> 'strength.'	} In all these words the <i>b</i> has not yet been satisfactorily explained. They may not be I.-E., but if they are, it is impossible to give satisfactory cognates.
<i>bali-</i> 'offering.'	
<i>balbaja-</i> 'a grass.'	
<i>bila-</i> 'a hollow.'	
<i>bilma-</i> 'a wedge.'	

mala- 'dirty garment.' Probable cognates are *μέλας*, Latin *malus*, Armenian *mel* 'sin,' Lett. *melu*, Lith. *milyti* 'to err,' cf. Fick I.⁴ p. 109. He adds however cognates from a really distinct root, as it is unlikely that Lith. *malti*, Gk. *μύλλω*, Latin *molo*, &c. belong to a root meaning 'black.' Possibly *μέλεος*.

Hübschmann (*Arm. Stud.* I. pp. 42 and 73) prefers to identify *mel* with the root *meṛ* in *ἀμαρτάνω*, &c. The root *meṛ* is preferable: for Arm. *λ* represents I.-E. *ḷ* not *ṛ*.

None of Hübschmann's examples on p. 73 is certain for $\lambda = r$. In *astλ* beside *ἀστήρ*, *neλ* = O. S. *naru*, we may be dealing with I.-E. doublets—possibly due to formative suffixes. In *geλmn* we are dealing not with the root *meṛ*, whence *φέριον*, but with the root *meḷ*, whence *vellus*, *tāna*, *οὔλος*. As for *elbair* = *frater*, and *albeur* = *φρέαρ*, the position of the liquid shows that the words have a peculiar history. It may be noted

that *r* does not seem ever to precede *b* in Armenian—for in *arbaneak* and *arbenam* the *ar* = *r*.

mīl- 'to close the eyes.' Although this root only occurs once in R. V., it is vouched for by the more frequent *miṣ*-, which as an example of Fortunatov's law will meet us below. *mīl* cannot be referred to Lat. *mīror*, nor to *μειδιάω*. Possibly we should recognize a root *meil*- 'to soothe' or 'to close the eyes' according as mental or physical sense predominates. This adds *μείλ-ια* 'gifts' and *μείλ-ιχος*. Cf. also *σμῖλαξ*, *μῖλαξ*, 'the convolvulus,' which closes its flowers at night.

mlā- 'to soften' is a root of the same type as *glā*- (*supra*, p. 229), and again refers us to *M. U.* I. 1 (see esp. p. 53). It may then be analysed *ml̥-ā**, and the root will be *ml̥**, Lat. *molo*, Arm. *malem*, where possibly the correspondence of Latin *o* and Arm. *a* may prove *ml̥* in the *o*-scale according to Bartholomae's rule (*B. B.* xvii. pp. 91 sqq.).

This root in the form *ml̥* gives Skt. *mṛ* and in the form *ml̥̄*, *mūr*. It is therefore probable that the parallel root *mer*- postulated by Fick (I.⁴ p. 107) had no existence. Gk. *μάρναμαι* of course can equally well come from *mṛ̥* 'to die.' Skt. *mar* is gunated from *mṛ* and *mr-ad*- is also a Sanskrit development. Zend *mrā*- = *mlā*- (*Seldner B. B.* xvii. 349) of course proves nothing. At the same time a root-doublet *ml̥*:-*mṛ̥*- is not in itself impossible. See pp. 256 and 260 f. *infra*.

mluc- is a derivative of the last mentioned root and will be further discussed under the exceptions.

lip- 'to besmear.' For this word the *l̥* is proved by Lith. *limpu*, O. B. *lēpu*. The question of its relation to *rip* belongs also to the section on irregularities.

libujā-, 'a liana, creeping plant.' Clearly not Indo-European.

lubh- 'desire eagerly,' cf. Lat. *libet*, *lubet*.

lopāca- 'a fox, jackal,' I.-E. *loypēko*. See *Fox and Wolf* [p. 92 f. *sup.*] for cognates and further analysis.

b. The words with suffixal *l* need no separate comment and are taken as a class, below p. 256.

Before discussing C any further it is as well to take the small number of words in which the *l* immediately precedes a labial into consideration.

D. Words in R. V. which contain *l* immediately preceding a labial explosive.

<i>kalp-</i>	<i>kulpha-</i>	<i>talpa-</i>	<i>çilpa-</i>
<i>kilbiṣa-</i>	<i>jalpi-</i>	<i>balbaja-</i>	<i>ulba-</i>

Of these *kilbiṣa-* and *kulpha-* have already been discussed: in the former *ḷ* was shown to be original and sufficiently explained by the *k*, while the latter was shown to have no satisfactory derivation, but at least no suspicion of I.-E. *ṛ*. Again *balbaja-* was regarded as non-Indo-European. These may therefore be passed over.

kalp- *supra*, p. 225.

jalpi- 'whispering.' Later Skt. *jala* 'water' might possibly suggest a derivation, but according to Per Persson (p. 111) *jala-* goes with *quellen*, which brings us back to *ge-ḷ*. Of this we have seen several derivatives for Vedic but none that approaches *jalpi-* in sense. A more simple plan is therefore to refer it to the root *jar-* 1 as Grassmann does. For *-p* as a determinative cf. Persson *op. cit.* p. 49 f. This then refers us back to the wide-spread root *ḡer-*, Gk. *γῆρίω*, Lat. *garrio*, so that *jalpi-* will represent *ḡerp-* (*ḡerp-* if we add Lith. *garsas*, O. H. G. *queran*, Fick I.⁴ p. 35).

talpa- 'couch,' 'lair,' is obviously *torpo-s*, the regular *-o-* noun from the root *terp-* whence *trp-* and *τέρπω*. It is therefore 'the place where one takes one's pleasure,' and is of special interest as giving just the necessary sense-link to connect *τέρπω* and *torpeo*, which would otherwise have seemed fanciful.

çilpa- 'gay.' This sense is post-Vedic and secondary. It is properly a substantive and means 'shape' or 'form,' as is shown by the Vedic compound *suçilpa-* 'with beauteous form.' This suggests a relation with Latin *corpus*.

We saw above that the superficially accurate identification of *corpus* and *kyp* is impossible on a closer testing; for *corpus* cannot be either **querpus* or **quorpus* whether from **qor̥p* or **q̥rp*. Accordingly Mr Wharton's parallel of *πραπίδες* with its sense-difficulties is also dismissed. Few Latin words are less ambiguous in pointing out their origin than *corpus* with its stem *corpor-*. By the regular phonology of the language it must represent *k̥or̥pos-* or *k̥r̥pos-*, and as we thereby get a neuter *es/os* stem, the former is at once excluded by the laws of I.-E. ablaut. The neuter stem *k̥rp-es-* is of course regular, whether we suppose that *βάθος πάθος* &c. prove an I.-E. *-es-class* with Tiefstufe or whether, as is much more probable, we suppose original ablaut between root and F. S., as

<i>βενθ-ος-</i>	<i>k̥erp-os-</i>	<i>temp-os-</i>	<i>gēn-os-</i>
<i>βαθ-εσ-</i>	<i>k̥rp-es-</i>	<i>tmp-es-</i>	<i>g̃ñ-es-</i>

variously levelled as *βένθος*, *βάθος*, *corpor-*, *tempor-*, *gener-*. This root *k̥erp* in the form *k̥erp-ό-* is precisely what is wanted to explain *εἰλπά-* (oxytone *ό-*stems, see G. Meyer *Gr. Gr.*² § 17), both in form and sense, so that the identification is at least tempting.

ulba- 'egg-shell' has been reserved to the last because it is ambiguous. In the first place the *b* may point to a non-I.-E. origin. If not, and perhaps this is the more probable view, it must be regarded as one of those unexplained cases where *b=γ*. But again as regards the root. There are two wide-spread and very similar roots *uel-* and *uer-*. Both of these go back to the *subjective* period of the language (see *Fox and Wolf* sub init.) when a phonetic group expressed all ideas in the same mental system. Thus the wool of sheep, clothing, wrapping, and drawing on (garments) all belonged to the same and were all expressed alike. Hence all these senses are traceable in different developments of the same root.

Thus from *uel-* we find the sense of 'wool' present in Latin *vellus*, *tāna*, Germ. *Vlies*, our *wool*, Arm. *gelmn*, Gk. *οὔλος*, Skt. *ūrṇā*, Lith. *vilna*. The sense of 'covering' is to be found in Gk. *εἶλω*, Skt. *vr̥*, &c. The sense of 'wrap' in *εἶλω*, *ἀλείς*,

volvo, Arm. *gael*. Of 'pluck, pull, &c.' in Lat. *vello* (= *vel-no*), and in the extended form *velq-* (see *Fox and Wolf*).

Similarly from *uer-*: the sense of 'wool' in *ἔριον*, **φρην*, and *φάρνός*, Arm. *gairn* 'lamb,' Skt. *urāṇa* [and *ūrṇa*]. Of 'covering' in Skt. *vr-*, then developed into 'protect' in *φρύομαι*, *φρύομαι*, *ἐρύω*, Goth. *varjan*, and perhaps Lat. *vereor* (= **voreor* causative). Hence again Gk. *εἵρεπος* 'slavery,' Arm. *giri* 'a slave,' as *servus* to the root *ser-*: of 'draw' in *ἐρύω*, in Lat. *operio*, *aperio* (= *op-uerio*, *ap-uerio*), Lith. *-veriu*, Brugm. *Idg. Forsch.* I. 174. The reason for not referring Arm. *gelmn* and *gael* to the second root lies in the separation of Arm. *λ* from I.-E. *ṛ* (*supra*).

It would therefore seem that *ulba-* may or may not be an example of the rule that I.-E. *ṛ* becomes Vedic *l* before a labial explosive.

We have then *kalp-*, *jalpi-*, *talpa-*, and *ṣilpa-* offering fairly simple and plausible derivations if their *l* can = I.-E. *ṛ* and leaving us without any explanation if their *l* = I.-E. *ḷ*.

In each of these the *l* immediately precedes a *p*, and as the theory was that Vedic *l* (*r* depended on a dental) (lingual and not *ḷ*) (*ṛ* distinction, and that labial influence in some way favoured the dental at the expense of the lingual position in the case of I.-E. *ḷ*: it is not altogether improbable that in the combination *ṛp* the labial might similarly cause a retention of the dental position, and consequently produce Vedic *l*.

It is *not* logical to conclude that the same would have happened for *ṛq*; for, as we have seen, Armenian shows that before the separation *q* had become *kʷ* with the *ʷ* following and tending to separation from the *k*.

As however there is no word which *demand*s a similar rule for any other labial explosive, it is best to state the rule as narrowly as possible, viz. that "I.-E. *ṛ* before *p* becomes *l* in Vedic" and consequently elect to refer *ulba-* to the root *uel-*.

Having thus got rid of *kalp-*, *jalpi-*, *talpa-*, and *ṣilpa-*, and retained *ulba-*, *kilbisa-*, *kulpha-* and *balbaja-*, we can proceed to analyse the result of C.

Nine have no plausible etymology, viz. *kalmali-*, *kulpha-*, *plāṣi-*, *bala-*, *bali-*, *balbaja-*, *bila-*, *bilma-*, *libujā-*.

All the remaining eighteen have been shown to contain I.-E. *l*, or at least to have a probable case for it. Excluding all double representatives of possibly the same root we reduce these 18 to 15, of which number three are capable of coming under B.

The inference as before is that the labial is the cause of the *l*, and hence the other words for which no derivation has yet been found must also show *l*, if they are Indo-European.

§ 3. ON FORTUNATOV'S LAW.

Hitherto the only test to distinguish original *l* and *r* in Sanskrit has been provided by a law which Fortunatov propounded in *B.B.* VI. pp. 215—220 to the effect that while *r* + dental remained in Skt. *l* + dental invariably became the lingual corresponding to the dental in question. This rule is not accepted by Brugmann (*Grds.* I. p. 211 note), but has recently been defended by Bechtel (*op. cit.* p. 381 f.)¹. It is also accepted by Fick, and from the large number of cases in which it is found to work there is considerable difficulty in the way of rejecting it. The evidence is at all events so strong that no account of the liquids in Sanskrit can avoid touching upon it, and if we have made out a case for distinguishing Vedic *r* and *l*, it is all the more important to ascertain the relation between this distinction and Fortunatov's.

It is not contended that this law explains the Skt. linguals as a class, and hence we are not in a position to say when we have all the examples of the rule before us: more may be added from time to time to explain other instances of the lingual.

In attempting to fix the date of the change it is important to classify those examples which have been given of the rule

¹ [It is now discussed at length by Bartholomae in *I. F.* III. pp. 157—197. T.]

according as they are or are not to be found in the Ṛig Veda. These again may be conveniently subdivided at once, according as they do or do not obey our rules for the preservation of *l*.

I. R.V. examples of Fortunatov's law.

1. Containing *l*, a rounding velar, or a labial.

<i>ākhaṇḍala-</i>	<i>kunḍa-</i>	<i>pāṇi-</i>	<i>miṣ-</i>
<i>āghāṭi</i>	<i>jāṭhala-</i>	<i>pāṣia-</i>	
<i>kāṇa-</i>	<i>paṇi-</i>	<i>phaṇ-</i>	

2. Not containing either of these sounds.

aṇu-, *āṇi-*, *jāṭhara-*, *jaḍhu-*, *vāṇi-*.

II. Later examples.

1. Containing *l*, a rounding velar, or a labial.

<i>kāṇḍa-</i>	<i>khaḍ-</i>	<i>paṭ-</i>	<i>phaṭa-</i>
<i>kiṇa-</i>	<i>khoḍa-</i>	<i>paṭa-</i>	<i>bhaṣ-</i>
<i>kūṭhara-</i>	<i>guḍa-</i>	<i>paṭala-</i>	<i>māṇava-</i>
<i>kūṇi-</i>	<i>ghaṭa-</i>	<i>paṭu-</i>	<i>muṇḍa-</i>
<i>kunḍala-</i>	<i>ghaṭ-</i>	<i>puṭa-</i>	<i>laṣ-</i>

2. Not containing either of these sounds.

aṭ, *ṣaṭu-*, *jaṭā-*, *vaṭa-*, *vāṭa-*, *hāṭaka-*.

It is necessary briefly to discuss the etymology proposed for each of these words.

1. *ākhaṇḍala-* 'broken,' 'leaky,' is the only R.V. representative of **khaṇḍa-* which appears to be related to the later *khaḍ*. This Fortunatov connected with Lith. *skeldėti* 'to divide,' and the equation is plausible. Cf. also *kūṇḍa-* with the frequent interchange of tenuis and breathed aspirate¹.

āghāṭi 'cymbals,' is derived by Grassm. from later *ghaṭ* 'to come together,' which again F. connects with Goth. *fragildan*. This does not seem to have been adduced as a Vedic parallel, so perhaps Grassm.'s derivation is denied.

¹ [Cf. Bartholomae, *I. F.* III. pp. 173—4. *T.*]

kāṇa- 'pierced, one-eyed,' (A.V. and Bk. x.) with Lith. *kalti*¹.

kunḍa- 'a pot' with *κυνιδέα* is a very unsafe etymology, the more so as we have no reason for seeing an identity between Skt. *u* and Gk. *υ* (*F. and W.* p. 96 ff.). Further it only occurs in R.V. in derivatives and lastly *ṇḍ* is still an obscure combination².

jathala- 'belly,' is very probably Gothic *kilpei*³.

paṇi- 'miser,' is connected by Grassm. with the later *paṇa-* 'to traffic,' 'barter'⁴. These words may be discussed together here. Böthl.-R. distinguish $\sqrt{paṇ-}$ 'praise' and $\sqrt{paṇ-}$ 'sell.' Pischel in his *Vedische Studien*, p. 199, wishes to identify them, remarking that the sense of 'selling' can come very easily from that of 'praising' or vice versa. He would therefore refer both to the R.V. *pan* (not *paṇ*) and make that mean 'buy' (or 'sell'). In this he seems to have a belief in the sporadic appearance of dentals for linguals in Vedic, as he compares *paṇi-*, and therefore does not seem to question Grassmann's sense for it or his etymology (which are open to doubt). This is a dangerous and unsafe doctrine. Moreover the Lith. *pelnis* 'reward' proves that the sense of 'sell' must have been anterior to that of 'praise,' and therefore the development of sense must be reversed.

It is more scientific to refuse to connect Vedic *pan-* with the root *pel₂-n-* at all. It obviously would represent *pen₂-*, and this may be found in Latin *pendo*, whether *pen-izō* after Conway or *pen-dh-ō* after V. Henry. In that case the sense of *pendo* 'I pay' preceded that of *pendo* 'I weigh,' and not vice versa. This of course is hazardous: a more probable equation is with Greek *πᾶ-σάμενος*. Then Pischel's restoration of the sense 'pay' for *pan-* in R.V. would still hold.

Whether *paṇi-* is or is not connected with *pel₂-n-* must, in view of its meaning, remain doubtful.

¹ [v. *infr.* T.]

³ [*ibid.* p. 194. T.]

² [Bartholomae, *I. F.* III. p. 173.

⁴ [*ibid.* p. 180. T.]

T.]

pāni- 'the hand', Gk. *παλάμη*, &c., see Bechtel *op. cit.* p. 383 and *n.*

pāṣia- Gk. *πέλλα*², Germ. *Fels*, Bechtel l.c. 384 and Fick I.⁴ 83.

phan- 'to leap,' is a Vedic example which F. seems to have overlooked. It is very probably from *pheḷ-n-* and so an extension of the root discussed under *phala-* (*supra*, p. 236).

miṣ- is probably also an addition to his list. *V. sub mīl-* (*supra*, p. 237).

2. *anu-* 'fine, thin,' F. connects with *ἀλέω* 'to pound,' which raises the question of the origin of that word. Fick's identification of it with Latin *molo* is highly doubtful, as it is scarcely probable that *ἡλ-* could become *ἀλ-*; yet he clings to it in ed. 4 (p. 516). The Armenian words play an important part in the answer. They are *αλαμ* 'I grind' and *aleur* 'meal.' Now *λ* and *l* do not interchange in Armenian, although their precise range has not yet been defined, and Hübschmann points out that *aleur* can scarcely be *ἄλευρον* borrowed because Gk. *λ* is regularly transliterated by *λ*.

The explanation is probably this: *ἄλευρον* and *aleur* are connected with each other, but not *ἀλέω* and *αλαμ*. Verb and substantive are also to be separated in each language. We then get a faultless derivation for each.

a^xl-eg-ḡo- is a derivative from *a^xl-* 'nourish' (Latin *alo*). *ἀλέω* is shown by the aorist *ἠλέσσαι* to be *ἀ-λεσ-ω* with *ἀ-* prothetic. The root is therefore *les-*: perhaps compare Skt. *ṛs* 'thump,' 'stab' (or ? *rasa-* 'liquid'), and Latin *lēnis* for **les-ni-s*.

αλαμ by the regular loss of initial *s* is from *sa^xl* (perh. *sol-*: Lat. *solum*?), whence *salio ἄλλομαι* meaning 'to stamp.' While Latin and Greek developed the sense of 'jump,' Armenian developed that of 'pounding.'

The derivation of *anu-* from *ἀλέω* must therefore be rejected. Fick I.⁴ p. 123 has a different explanation in which he includes *āni-* *infra*. He makes them both come from *lⁿi-*, *lⁿā-* and a

¹ [Bartholomae, *I. F.* III. p. 193. *T.*]

² [*ibid.* p. 192. *T.*]

root *len-*, comparing O.H.G. *lun*, Goth., *af-linnan*, &c. This is of course absolutely impossible as *l* could not become *al* or *āl* in Skt.

*āṇi*¹. Fortunatov's equation with *ᾠλένη* does not give a plausible sense; for *āṇi-* means 'axle,' which is a very different thing from 'elbow.' Moreover *ᾠλε-* has its proper representative in *ara-tni*, which shows that the root was dissyllabic in Skt. also. The sense of 'thinning,' which Fick's derivation (above) would give to it, agrees well with its traditional application to the thin part of the axle on which the wheel revolves, and also to the sense, which Fortunatov quotes, of the leg just over the knee, where it thins. But we have seen that there are difficulties.

We must admit then that these two words—possibly connected and possibly with *l* lost—have not yet been cleared up.

jathara = *jathala* belongs of course to our list of substitutions of *-ra-* for *-la-*.

jaḍhu- 'stupid,' or *jaḷhu-* must be excluded from proof as the question of the origin of this *ḍ* is still uncertain².

vāṇī- 'a reed,' with Gothic *valus* 'a staff' (Bechtel, p. 384).

vāṇī- 'music,' with Polish *wołac* 'to call' (Fick⁴, I. p. 133).

Now there are several R.V. words which must be taken together; they are

1. *vāṇa-* 'an arrow,' 2. *vāṇa-* 'a pipe,' and 3. *vāṇa-* 'music.'
1. *vāṇī-* 'a reed,' 2. *vāṇī-* 'song,' 'music,' 'noise.'

It is scarcely possible to believe that these are to be separated from one another, and it seems equally impossible not to refer them *all* to *vā-* 'blow.' Hence at once 'reed,' then

{arrow and from 'pipe' or independently {music³
{pipe and song

The Vedic evidence for *l* + dental → lingual (except under the conditions for retaining *l*) is thus reduced to *aṇu-*, *āṇi-* and *vāṇi-*, for none of which has a certain etymology been

¹ [*ibid.* p. 193. T.]

² [*ibid.* pp. 178—9. T.]

³ [Cf. Johansson, *Idg. Forsch.* III. p. 252. T.]

offered. On the other hand *n* for *n* is the substitution of lingual for dental which may be considered fairly common for R.V. (Whitney § 46). We are therefore perhaps justified in asserting that it has not been proved that *n* may not be original for the above words.

Of course this is a very different thing from allowing that *n* can stand where the etymology requires *n̄* (p. 243, *supra*).

This gives at once a satisfactory account of *vāṇa*, &c. *supra*, and it suggests that *an̄u-* may possibly be *n̄ⁿu-* and go with Gk. *ἀνευ*.

Later examples.

kāṇḍa-, see *ākhaṇḍala-* *supra*.

kīṇa-. Lat. *callum*, but see on *khala-* (*supra*, p. 229)¹.

kūthāra- 'an axe.' Lat. *culter*, Lith. *kulti*. (Or possibly to the root *gol-* (*supra*, p. 224) as the *u* can be due to the *l* (cf. *kuṇi-*) and so Fortunatov—but then *kulti* is irregular².)

kuṇi- 'lame in the arm,' Gk. *κυλλός*. Here again *u* does not correspond to *υ* (see *F. and W.* p. 96 ff.)³.

kuṇḍala-, see *kuṇḍa-* *supra*.

khaḍ- 'split,' with Lith. *skeldeti* (see *ākhaṇḍala*).

khoda- 'lame,' Gothic *halts*. We seem to have here a false gunating of *khud-* where *u* = *l* (Fortunatov).

guḍa- 'a ball,' with Skt. *gola* and *γανλός*, Fröhde *B.B.* x. 298⁴. Fick prefers to make it *gl̥da-* and connect with *galḍā-* 'straining.' The sense-connexion I cannot see, and *gl̥da-* would become *gr̥da-* regularly⁵.

ghaṭ, see *āghāṭi* *supra*.

ghaṭa- 'a pot,' German *Gelte*. Surely rather a wide stretch.

¹ [Bartholomae regards *kīṇa* as a Prākṛit form for **kṛṇa*, *ibid.* pp. 159—161. *T.*]

² [Cf. Bartholomae, *ibid.* pp. 161—2 and 168, who regards *kūthāra* and *puṭa* as Prākṛit forms for *kṛthāra* and

pṛta. *T.*]

³ [*ibid.* p. 161. *T.*]

⁴ [Fröhde mentions, but does not adopt, this view. *T.*]

⁵ [Cf. Bartholomae, *ibid.* p. 175. *T.*]

paṭ- and *sphuṭ-* 'to split,' cf. Per Persson, *op. cit.* p. 33.

paṭa- 'a garment,' Slavonic *platino*. ?

paṭala- 'a roof,' Gk. *πέλτη*, O.N. *feldr*.

paṭu- 'sharp, bitter,' *πλατύς*, v. Fick I.⁴ p. 87¹.

puṭa- 'a fold,' Gothic *falþan*, our 'fold,' Gk. *πλάσιος*, cf. Fick I.⁴ p. 86 f.²

phaṭa- with *phal-*, see under *phala-* (p. 236 *supra*).

bhaṣ- 'to bellow' and *bhāṣ-* 'to speak,' Lith. *balsas*, Germ. *bellen*, our *bell*³.

laṣ- 'to desire eagerly' = *lals-* redup. from *las-* *λι-λαίωμα* and *las-civus* (perhaps the most convincing of all Fort.'s derivations)⁴.

māṇava- 'youth,' Prussian *malnyx* 'child' ??

muṇḍa- 'bald,' *muṇḍ-* 'to break,' *ἀμαλδύνω*, Lith. *muldyti*. As mentioned before *ṇḍ* is difficult⁵.

at- 'to wander⁶,' Fortunatov explained by *ἄλλη*. This is not to be accepted, because Vedic shows *at-*, and whereas the linguals encroach frequently upon the dentals, it is not proved that Vedic dentals replace linguals, as Pischel and Bechtel would maintain (v. Bechtel, *op. cit.*, p. 384). This derivation is accepted by Persson, p. 35.

cāṭu- 'friendly words,' Fortunatov compares Gothic *hulþs*, but *cāṭu-* clearly means 'flattering' i.e. 'deceitful,' and is connected with *caṭati* 'conceals.' This however is the later form of Vedic *catati*.

jaṭā- 'twine.' F. compared Lith. *galtinis*. Bechtel prefers Fröhde's equation with A.-S. *clīðe*, O.H.G. *chletta*. The former is better⁷ as involving no metathesis.

¹ [Bartholomae, I. F. III. p. 186. T.]

² [v. note on *kuṭhāra-*, *supra*. T.]

³ [*ibid.* p. 195. T.]

⁴ [*ibid.* pp. 195—6. T.]

⁵ [*ibid.* p. 173. T.]

⁶ [*ibid.* pp. 166—7.]

⁷ [*ibid.* p. 186. T.]

vaṭa- 'a cord,' Lith. *valtis* 'yarn,' Russian *voloti*, √*uel* 'wind!'

vāṭa- 'an enclosure,' Latin *vallum*. ?

hāṭaka- 'gold.' Fick I.⁴, p. 55, equates with O.B. *zlato*, Gothic *gulps* making the original form *zholto* or *zhlto* (i.e. *gholto* *ghlto*). The root then is *ghel* 'to be yellow' or 'green,' Latin *helvus*, Gk. *χλωρός*². Vedic shows the *r* in derivatives as *hari* (which might also be from √*gher*, *θερμός*, *warm*) and *hiranya-* which is of course regular.

To sum up, taking those derivations only which are tolerably safe, we find

kāṇa-, *jaṭhala-*, *pāṇi-*, *pāṣia-*, *phaṇ-*, *miṣ-* in R.V.

kīṇa-, *kūṭhara-*, *kūni-*, *khaḍ-*, *khoda-*, *guḍa-*, *ghaṭ-*, *paṭ-*, *paṭala-*, *puṭa-*, *phaṭa-*, *bhaṣ-*, *laṣ-*, *sphuṭ-*,

and

jaṭā-, *vaṭa-*, *hāṭaka-*,

in later Skt. The six examples from R.V. all obey the rules which cause *l* to be preserved: the later examples obey these rules in 14 cases and disobey them in three or possibly four.

Examples with *ṇḍ* have been purposely excluded. Then comes the question: when is Fortunatov's law not observed?

The exceptions must obviously be classified as Bechtel does (p. 385).

1. *l* + dental retained in Vedic;
2. *l* + dental becoming *r* + dental;
3. *r* + dental becoming a lingual.

The only example of the first class is *galdā-* which was discussed above. Pischel *Vedische Studien* I. 83 rejects that derivation and Grassmann's sense, and substitutes one which makes it an adjective from *glah-* = *gardh-* (!). The word *galda-* occurs once in R.V. and once later, which latter passage P. has to emend to save his interpretation. It is dangerous to be original at so great expense, and as he fails to explain the *l*

¹ [Bartholomae, *I. F.* III. p. 190. *T*]

² [*ibid.* p. 186. *T.*]

except airily as 'dialectic,' Bechtel is not to be congratulated on shelving *galdā-* with a mere reference to him.

Now it may be granted that Vedic and classical Sanskrit represent different though closely cognate dialects; but to say that the Vedic poems are themselves in a hybrid dialect is a very different thing. An explanation of a Vedic form as 'dialectic' is therefore ipso facto condemned, as even the irregularities of Book x. may largely be natural developments if not entirely, and *galdā-* does not belong to Bk. x.

There are three ways in which *galdā-* might be explained without any sacrifice of principle—two of which are applicable to Pischel's rendering, but until it is better established, there is no good reason for departing from the traditional explanation.

a. The root *gel-* was extended by *-d* after F.'s law had acted. *gal-* would be the Skt. form by our rules. For *-d* as a root determinative cf. Per Persson, pp. 35 ff. If it be denied that roots could be so modified as late as this, the formation could still be analogical: cf. *mrād-*.

b. *galdā-* was never severed mentally from its simplex and hence retained the *l* by 'Systemzwang.' This applies equally well to Grassmann's and to Pischel's theory.

c. **gardh-* and **gladh-* were parallel roots: **gardā* belonged to *gardh-* and received its *l* from being wrongly referred to *glah-*. Pischel seems to hint at this view on p. 83.

For the present, the first of the above explanations may be considered the most satisfactory.

For exceptions of the second type Bechtel adduces

Skt. *jartu-*, Gothic *kilpei*.

Skt. *mardhati* 'neglects,' Gk. *μαλθακός*.

Skt. *mūrdhan-*, A.-S. *molda* 'head.'

The last two examples Bechtel tacitly explains by the first, which he treats as follows: 'Vedic shows *jāthara-*, later Skt. *jartu-*: therefore they belong to different dialects, one of which represented *lt(h)* by *t(h)* the other by *r(t)*.' On this there are

one or two remarks to be made. In the first place that later Skt. came from a dialect which gave *r* for *l* before a dental is worse than no explanation of the other *two R. V. examples*. In the second place Fortunatov's law is no law for Vedic alone but also for classical Skt., so that on these three words Bechtel must be understood to build four dialects thus

R.V.		Skt.	
† &c. dialect	rt &c. dialect	† &c. dialect	rt &c. dialect
pāṇi- &c. &c.	mardhati	laṣ- &c. &c.	jartu-
	mūrdhan-		

to say nothing of the *galdā*- dialect which is a fifth¹.

It may be possible not to proceed to such lengths, if we consider each example separately. *mūrdhan*- disappears at once as the *ūr* can obviously = $\bar{l}$, for which F.'s law does not hold: and this is also the explanation of *pūrṇa*- (and *ūrṇa*- if from *uel*-) and not Bechtel's 'Systemzwang.' As for *mardh-ati* = *μαλθακός*, nothing can be simpler than to suppose as Per Persson does (*op. cit.* p. 46), that *mardh*- is gunated from *mrdh*- = *mḍdh*-². It would however be possible also to proceed as follows: *μαλθακός*, *ἀ-μαλδύνω*, *μαλακός*: *mardh*-, *mrdh*-, Arm. *mełk'*, and O.H.G. *milti* show that the root was *meł* with various extensions. Then it is only necessary to refer to the parallel root *meṛ*- (p. 236—7) to explain *mardh*- as *meṛ-dh*-.

Lastly *jartu*- beside Goth. *kilpei* and Skt. *jaṭhara*-; and first as regards the breathed aspirate, Feist is probably right in comparing *niu-klaḥs* 'new-born,' and hence analysing *kilpei* as *gel-t*-. Then the *-th*- is alternative, see Per Persson pp. 28 ff. Now, as we saw above, *jaṭhara*- must be a less original form than *jaṭhala*-, which accounts for it: *jartu*- and *jaṭhala*- must therefore be compared and we get

gel-t- [Gothic *kilpei*, Gk. *δέλτα*] → *g'elt* → *jart*- → *jartu*-,
gel-th-o-lo- → *g'eltʰoło* → *jaṭhala* → *jaṭhala*,

with the exact distinction required by our rules.

¹ [Of. Bartholomae, *I. F.* III. pp. 158—9. *T.*]

² [It may be doubted whether Per Persson means precisely this. *T.*]

Exceptions of the third order adduced by Bechtel are

kaṭa- 'mat,' O. Pruss. *korto*.

kaṭuka- R. V. 'sharp,' Lith. *kartus*.

kāṇa- 'one-eyed,' O. B. *krūnŭ*.

Of these the 1st and 3rd are too hazardous to prove¹ anything, while for the second the solution is almost certainly that which Fortunatov himself points out; that beside *kart-* existed a root *kalt-*. These we can now write I.-E. *qel-t*, *qex-t*. The actual presence of R. V. *karta-*, *kartana-* makes this almost certain². Bechtel prefers to increase his number of dialects.

What then are we justified in concluding as to the relative dates of Fortunatov's law and the changes of *r* and *l* that we have assumed? Vedic and classical Skt. may again be taken separately. For the former we have found that all the good examples and even all that are moderately probable would have *l* preserved by our rules. Thus F.'s law might have acted subsequently. Further if *ākhaṇḍala-* and *jāṭhala-* be taken into account the supposition that it *did* act subsequently explains the *l* which is otherwise irregular.

For later Sanskrit we find that the great majority of the examples, again including the most certain, fall under our conditions; but that for three words the derivation proposed necessitates the supposition that it acted earlier, while three others *laš-* *paṭala-* and *jartu-* necessitate the supposition that it acted later. The evidence for and against therefore amounts to this

1. Fortunatov's law was anterior to l becoming $\begin{Bmatrix} l \\ r \end{Bmatrix}$,

then *a.* The examples which would show Vedic *l* by rule are only accidentally numerous.

b. *jāṭhara-* is regular.

c. *ākhaṇḍala-* *jāṭhala-* *paṭala-* have substituted *-la-* for *-ra-* (see p. 256 ff.).

¹ [Cf. Bartholomae, *ibid.* p. 195 (*kaṭa*) and pp. 169, 188, 193 (*kāṇa*). T.]

² [*ibid.* pp. 190 and 195. T.]

d. *jaṭā- vaṭa- haṭaka-* have their derivations saved.

e. *laš-* and *jartu-* are irregular.

2. Fortunatov's law acted after *ḷ* had become $\begin{cases} l \\ r \end{cases}$,

then a. The reason why twenty examples obey the rules for preserving *l* is explained.

b. *jaṭhara-* is irregular and belongs to p. 256 ff.

c. *ākhaṇḍala- jaṭhala- paṭala- laṣ-* and *jartu-* are regular.

d. The derivations of *jaṭā-, vaṭa-, haṭaka-* must be given up.

All that can be said then is that the weight of evidence is in favour of supposing that F.'s law acted after the division of *ḷ* into *l* and *r*. It would seem also that it was somewhat probable that it acted before Vedic and classical Sanskrit (or rather their prototypes) had separated.

NOTE. Windisch's examples of this law in *K.Z.* (xxvii. 168) have not been included; for, as Brugmann says of Fortunatov's, they are "etymologisch sehr unsicher" and moreover are *all* examples of *ṛ*.

In particular *sthūṇa-* may equally well be for *sthū-na-*¹; *ṇa-* can hardly go with ἄλλος and Arm. *en*, for the *e* is irregular; more probably

ṛi-

we have a derivative of *ṛi-no-* 'the runner'²; the other examples hardly rise above a guess.

§ 4. EXCEPTIONS. CHANGES IN PERIOD V.

We have now to consider the more striking points of the evidence which may be urged against the above account of the liquids. This it will be convenient to arrange under two heads which correspond to the Fifth and Sixth Periods of our introductory narrative: the Fifth Period including those modifications of the natural state of things which intervened between the changes of the Second and Third Periods and

¹ [Bartholomae, *I. F.* III. pp. 170—1. *T.*] ² [*ibid.* p. 172 and note. *T.*]

the composition of the latest R.V. hymns, the Sixth those which are found in classical Sanskrit.

R. V. Exceptions.

These naturally are of two kinds : those which show *l* when by our rules they ought to show *r*, and those which show *r* when by the rules they should show *l*.

As before those in which the *l* is apparently suffixal are distinguished from those in which it is radical.

I. *l* instead of *r*

a. <i>jalāṣa-</i>	<i>vala-</i>	<i>ṣalya-</i>
<i>lāya-</i>	<i>valṣa-</i>	<i>sālāvṛka-</i>
<i>lodha-</i>	<i>velasthana-</i>	
b. <i>akṛṣīvala-</i>	<i>madhula-</i>	<i>ṣūla-</i>
<i>kevala-</i>	<i>-miṣṭla-</i>	<i>stolā</i>
<i>caṣāla-</i>	<i>vidvala-</i>	<i>syāla-</i>
<i>tr̥dila-</i>	<i>vṛsala-</i>	
<i>bahula-</i>	<i>ṣiṣūla-</i>	

jalāṣa- means 'healing.' The form may not be Indo-European. If it be, the *ṣ* excites a suspicion that *l* may have been lost before it by Fortunatov's law. In that case the form implied will be *ḡeṛ-*, *ḡeḷ-*, *geṛ-* or *geḷ-* *a^xlso-*. This hints at a possibility of a noun *ḡeṛa-*, cf. 3 *jar*, meaning 'illness' compounded with the root *ol* 'to destroy' and the suffix *so-*: so that the sense will be 'illness-destroying.' Then the *l* falls under class A and is an additional example to prove that Fortunatov's rule acted at a later date.

lāya- 'a missile.' Grassmann is probably right in deriving from *rī* 'set in motion,' and the variation is interesting. From a root *reḷ-* (cf. *rej-* and Per Persson *op. cit.* p. 15) the I.-E. reduplication would be *re-roḷ-* for the pf., whence would come regularly *lale-*. Such forms could not persist in system with the normal *r-* forms, but they seem to have existed long enough to produce *lāya-*. Confirmatory to some extent is the later intensive from this root *lelīya-*, *lelāya-*, by reduplication *ṛai-ṛī-* (Fick I.⁴ p. 115 *rej-rei*, but cf. *δαιδάλλω*, *αίσσω*, &c.). Hence also *lāya-* and *alāyia-*.

lodha- 'a reddish animal' from the root *ṛeṇdh-* rests probably on decomposition. It so happens that we may have preserved for us in Bk. x. what enables us to guess at the compound in question. *sātāvrka-* is a compound of *vrka-* with an irregular *l*. If an earlier compound **sātālodha-* had existed it explains both *lodha-* and *sātā-* at once.

vala- 'a hole.' The derivation from *vr-* i.e. *vel-* is very doubtful. The most frequent sense of the word is as the name of a demon, and proper and mythological names generally resist etymology. Indeed few of them can have been I.-E.

valṣa- 'a twig' is only retained in compounds, and therefore we may suspect that in some one of them *l* was retained by rule. Indeed *sahasravalṣa-* may be a modification of **sahasla-valṣa* to suit *sahasra-* and its other compounds.

ṣalya- in x. stands for *ṣarya-* of the earlier books. It is a derivative of *ṣara-* 'reed,' and means 'an arrow.' This is a good instance of the uncertainty of use which had come to pass when Bk. x. was composed. The *ṛ* is proved by Gothic *hairus*.

sātāvrka-, see under *lodha-* *supra*.

The second class of exceptions cannot be given with anything like completeness, because, it having been hitherto permissible to identify any Skt. *r* with European *l*, many rash derivations have been propounded which must now be given up. Reference may be made to Fick's *Wörterbuch passim*. There are however a certain number of derivations which deserve mention either from their own plausibility or from their frequent repetition. Such are

<i>aṅgiras</i>	<i>prath-</i>	<i>bhrama-</i>	<i>sphur-</i>
<i>āpra-</i>	<i>pru-</i>	<i>bhrāj-</i>	<i>raghu-</i>
<i>kruṣ-</i>	<i>pruṣ-</i>	<i>rakṣ-</i>	<i>rip-</i>
<i>cakra-</i>	<i>bhargas</i>	<i>ramb-</i>	<i>mruc-</i>

The only *necessarily* irregular forms with suffixal *-ra-* *ri-*, &c. are those which contain *r* or *l* in the body of the word. These will be discussed below.

aṅgiras: ἄγγελος is one of those superficial derivations of mythological names which used to be popular. They are nearly always certain to be wrong and may be left to mythologists.

āpra- 'busy,' is not from *ā* and *pr* as Grassmann says. That would require *ā-pl-o*, whence *āpla-* would be regular. Better therefore *āpra-* = *ōp-ro-*, $\sqrt{op}$ in *apas*, Lat. *opera*, &c.

It seems from Feist that this has already been proposed by Kluge, K. Z. xxv. p. 312, who adds Gothic *abra*. See Feist s.v.

kruç- 'to scream,' beside *kloça-* seems absolutely to disprove the rule. It is clear however from Gk. κρώζω beside κλώζω and κλώσσω and Lith *kraukti* that there were two parallel roots $\overset{\bar{k}}{qleuq(g)-}$ and $\overset{\bar{k}}{qreuq(g)-}$, which give *kloça-* and *kruç-* respectively; for the variation of palatal and velar at the end of roots is too frequent to require Fick's "Wirkung der labialen Anlaut" or "Ausgleichung" to account for the Balto-Slavic forms.

cakra- 'a wheel,' cannot be regularly from *qeqlo-*, and we might be tempted to adopt Fick's root *qeq-* (I.⁴ pp. 21, 22) and make it *qeq-ro-*. There is however an easier explanation, as in Skt. the etymological connection of *cakra-* and *carati* was never lost. Cf. e.g. R.V. III. 61. 3 *caraṇīyāmānā cakrām iṃa*. But in *carati* the *r* represents *ḷ* regularly. Hence *cakra-*.

prath- 'to spread out,' is generally connected with Lith. *plotas*, Gk. πλατύς, our *flat*. The sense however is not identical and probably we should see in *prath-* a similar formation to that described for *pl-ā-t(h)-* by Per Persson p. 33, but instead of being from the root *pl-* 'be flat,' as coming from the root *pr-* 'extend,' cf. πέρας &c., and probably παρά and περί, Latin *per*.

pru- is not identical with *plu-*, although the latter occurs only in Bk. x. (but cf. *plava-*). They are distinguished in Skt. both in sense and form. For the *ṛ* in *pru-* see Per Persson, p. 125.

pruṣ- and *pluṣ-* are a similar doublet.

bhargas 'brilliance.' In this word the *g* is probably not original as the cognates show $\hat{g}$, cf. *Grds.* I. p. 344, § 467 (1). (At the same time Lettish *birga-* may be remembered.)

bhrama- 'flickering motion,' has no connexion with Lat. *flamma*. The root is *bhr̥-em-* (cf. Per Persson *op. cit.* p. 68 for cognates).

bhrāj- 'to lighten,' is not to be directly connected with $\phi\lambda\acute{\epsilon}\gamma\epsilon\upsilon$. Rather Skt. *bhrāj*, *bhrāç*: *bhlāç*, Goth. *brahv*, M.H.G. *brehen*: $\phi\lambda\acute{\epsilon}\gamma\omega$ (Fick, pp. 93, 94) show that we have here again a root varying as *bhr̥- $\check{a}^{\times}\hat{g}$ -*, *bhl̥- $\check{e}\hat{g}$ -*.

rakṣ- beside $\acute{\alpha}\lambda\epsilon\xi$ - is only an apparent exception, as *kṣ* here represents $\hat{k}$ -s.

sphur- represents *sph $\hat{l}$ -*, and is therefore regular.

raghu- is generally referred to $\epsilon\lambda\alpha\chi\acute{\upsilon}\varsigma$. For the slightness of the evidence on which the form $\epsilon\lambda\alpha\chi\acute{\upsilon}\varsigma$ itself rests see *Fox and Wolf* (p. 100). The root *l $\check{n}$ gh-* however undoubtedly existed, as it gives $\epsilon\lambda\alpha\phi\rho\acute{o}\varsigma$, Goth. *lungar*, English *limber*. Besides this however must be assumed a form with $\check{r}$ or indeed more probably a distinct form *r $\check{a}$ gh* (*r $\check{o}$ gh*) to account for *raghu*. This is proved by Armenian *arag*; for Arm. *r* cannot = I.-E. $\check{l}$ and Arm. *a* does not = $\check{n}$.

rip-. Gk. $\acute{\alpha}\lambda\epsilon\acute{\iota}\phi\epsilon\upsilon$ is distinct: $\lambda\acute{\iota}\pi\alpha$ may be connected. If so, we must again assume double roots.

mruc- would be irregular from *mel̥-* beside *mluc-*; but as a matter of fact it comes from the root *mer̥-* of $\mu\acute{\alpha}\rho\nu\alpha\mu\alpha\iota$ and *mrad-*. It is therefore regular.

§ 5. EXCEPTIONS FROM OTHER CAUSES.

a. On the Suffixes *-la* and *-ra*.

Up to this point the words in which *l* appeared to form part of a suffix have been passed over and they must now be

considered as a class. The lists for the most common suffix *-la-* (including *-lā-*) will be given *in extenso*, as what holds for it also holds for the less common *-li* and *-lya*.

We find then

kīlā-, *tilvīlā-*, *lāṅgālā-*, *salīlā-*, *jāṭhālā-*, *paṭālā-*, and perhaps *caṣālā-* (7)—where *-la-* is preceded by *l*,

kīlā-, *kūlā-*, *khrgālā-*, *khargālā-*, *khela-*, *daṣāṅgālā-*, *puṣkālā-*, *-maṅgālā-*, *mudgālā-* (9)—where *-la-* is preceded by a velar in the same syllable,

apālā-, *udumbālā-*, *upālā-*, *kapālā-*, *tṛpālā-*, *pipālā-*, *mūlā-*, *viṣpālā-*, *ṣabatālā-*, *ṣīpālā-*, *ṣimbālā-*, *supippālā-* (12)—where *-la-* is preceded by a labial in the same syllable,

and the 13 above (p. 253) enumerated in which *-la-* is not preceded by any of these sounds.

There are therefore 28 regular forms to 13 irregular, which is a very much larger proportion of irregularity than has been found in any other part of the subject, and the inference is that there must be some special reason underlying it.

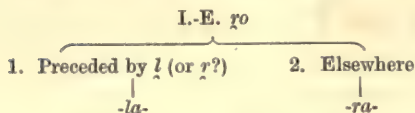
This reason becomes plain as soon as we turn our attention to the parallel suffix *-ra-*, which greatly surpasses *-la-* in frequency even in the R.V. Indo-European, as is well established by the Western group, had both *-ṛo-* and *-ḷo-* as very common suffixes (cf. *Grundriss*, II. pp. 169 ff., 186 ff.).

If anything, *-ḷo-* was perhaps the more common; and the greater frequency of *-ra-* in Skt. has hitherto been regarded as a mere instance of that assimilation of the two sounds which Zend carried through completely. Such a position is no longer tenable if Vedic *l*, *r* are as regular as we have tried to show.

In fact just as *l*, *r* in Vedic no longer differ as = I.-E. *ḷ*, *ṛ*, but according to their phonetic environment, so *-la-* and *-ra-* will no longer correspond in distribution to I.-E. *-ḷo-* and *-ṛo-*.

By our rules they will become distributed as follows,

I.-E. <i>lo</i>			
1. Preceded by <i>ḷ</i> , <i>ṛ</i>	2. By velar	3. By labial	4. Elsewhere
 <i>-la-</i>	 <i>-la-</i>	 <i>-la-</i>	 <i>-ra-</i>



We thus have four cases which would produce Skt. *-la-* to two that would produce *-ra-*, but the two latter would include very many more examples than all the rest put together. Besides, as our precise statement of the conditions which for convenience we call 'in the same syllable', involved the non-intervention of any explosive between the influencing and the affected sound, it follows that the very common I.-E. F.S. *-tlō* would always become *tra*, which indeed is the case.

Now principle obviously dictates that, where two suffixes existed side by side (and not 'dialectically') in I.-E., each had its own definite meaning, and there would be some roots the sense of which would permit derivatives to be formed with each suffix. Suppose that such a pair of derivatives was formed from a root containing a final labial (or labial + sonant final). Then in Skt. it follows that the derivative with *-ro-* would show *-ra-*, and that with *lo-* *-la-*. So with a velar (rounding). But under all other conditions the two would become identical (see however *infra*): in the case of a root containing a liquid both becoming *-la-*, in all the remaining cases both becoming *-ra-*.

Lastly *-la-* would appear abnormally in certain forms through being retained in a compound (or derivative) and then de-compounded (or de-derived).

In the R.V. we find

<i>apāra-</i>	beside	<i>apālā</i> ²
<i>upara-</i>	"	<i>upala-</i>
<i>trpra-</i>	"	<i>trpala-</i>
<i>puṣkara-</i>	"	<i>puṣkala-</i>

to which may be added the following.

¹ [See p. 232 sup.]

² A proper name, and so open to suspicion; but the sense suits.

mūla- beside *mūra-* obviously shows that the analysis *mūr-a-* is wrong. Both words mean 'root,' and the connexion with the root *meṣ-* (Lat. *moveo*, Gk. *ἀμείω*) meaning 'press' is obvious. It is true that *mūla-* only occurs in x., whence it might be referred to the time of confusion; but *sahamūla-* preserves the form in the earlier books (III. 30, 17), and shows that both existed side by side.

Add that *sīlamāvat-* is a derivative (Bk. x.) of *sīra-*, that *silikamādhyama-* may point to a form **sira-* or **siri-*, with which it was once connected, but which is now lost, and that *nīla-* we have seen to be decomposed from *nīla-lohita-*, but postulating a form *nīra-*; add that all distinction in force between the suffixes had of course been lost during their partial fusion, and it is not to be wondered at that *-la-* *-ra-* came to be felt as alternative and in different suffixes, and hence we find

miçla- irregular beside *miçra-* regular
-vala- „ „ *-vara-* „

and so on for all the large number of unwarranted *-la-* forms enumerated above.

Thus the proof which can be drawn from the forms preserved is sufficiently strong, and it is hardly necessary to urge that many more doublets may have contributed.

There is however another way in which the I.-E. double suffix would assist the result which is worth pointing out, as it illustrates a principle of language. It is this: let us suppose exactly those processes to have taken place which we have constructed, but let I.-E. possess the suffix *-lo-* only. Then this in Skt. subdivides into *la* and *ra* according as it is or is not preceded by the conditions we have mentioned. Now in such a state of things it is true that *ra* and *la* might have encroached upon each other, but it is more probable that the relation "(labial) . . *la* : (dental) . . *ra*," &c. &c., would have become or remained an unconscious but prevailing principle.

Such a result was effectually prevented by numbers of words like *vipra-*, *āpra-*, *çubhra-*, *akra-*, *agra-*, *vigra-*, &c. &c. (in which *-ra-* represented I.-E. *-ro-*), which would at once destroy any mental classification of the kind.

b. Classification.

Changes in language, taking place unconsciously as they do, are sometimes systematic and thorough. That is to say the pronunciation of a certain sound may change uniformly and that change may be totally uninterfered with by other influences, such as its neighbouring sounds. Among such changes the passing of *a*, *e*, *o* into *ā* in Skt. may be taken as an instance. The operation of these rules can never be interfered with by analogy, for analogy means the smoothing away of differences.

But the majority of changes in language are limited in their action, affect sounds under certain conditions and not under others, and so give rise to apparently illogical distinctions which analogy in all its forms steps in to sweep away. The more absolutely phonetic the change and the less connected with obvious reasons, the stronger is the influence of analogy. Thus in Greek *πέφαται* seems an utterly irrational perfect to *θείνω*, and such instances are innumerable.

Similarly in the case we are discussing the change in question is interfered with by a purely phonetic influence, and the inconsequence of the results would infallibly lead, and lead at once, to analogical levelling. Indeed the comparatively small amount of serious irregularity that exists in the earlier Vedic hymns forms a strong additional argument for their high antiquity.

These modes of analogical interference have already been frequently referred to, but for the sake of clearness they are here brought together.

1. The apparent capriciousness of the suffixes *-la-* and *-ra-*, which has just been explained, accounts for their complete confusion as also for that of *-li-* and *-ri-*; this doubtless was also an important incentive to interchanging the sounds in other conditions.

2. Very similar both in origin and result is the confusion which arises from the existence of I.-E. roots differing only in this, that one contained *ḷ* the other *ṛ*. These 'dual' roots are exactly on a par with the 'dual' suffix *ṛo-*, *ḷo-*, and, as in their

case, the variation had probably some original force which we are not now able to trace. The variation was *not* dialectic; for both forms co-exist, and it cannot be said that one is prior to the other. A considerable number of such roots have already been clearly proved (see *ṛeḷ* and *ṛeṣ* *supra* pp. 239, 240), and probably more will be found by degrees, but those only are important for our purpose which contain a labial or a rounding velar. Of these in accordance with our rules the *ṛ*-forms would show Skt. *r*, and the *ḷ*-forms Skt. *l*. Thus *ṛeṣp* *rup* and *ḷeṣp* *ḷup*- (cf. *Fox and Wolf*, p. 92 and n.) show Skt. *rup*- and *lup*- respectively—*qḷeṣk* *qṛeṣk*- give *kloṣa*- and *kruṣ*- (p. 255)—*plu*- and *pru*- give *plu*- and *pru*-, and so perhaps *lip*- and *rip*-, *rabh*- and *labh*- are to be explained: cf. also *mruc*- and *mluc*-.

3. *Reduplication* would play an important part in producing irregularity: all roots with *ḷ* initial would necessarily have *ḷeḷ*- in the perf., which would give Skt. *lal*-, although from a root with initial *r*. These would of course necessarily and at once be severed, new perfects with *rar*- (e.g. *rarabh*-) would at once be formed to the *r*-roots, and the *lal*-forms would either disappear or give rise to fictitious roots with *l*. Such we supposed to be the case of *ri*- : *li*- on the testimony of the later *leḷiya* and Gothic *reiran* which shows the *ṛ*. Where the *ḷ* was not initial, the effect would only be produced by "complete" reduplication, as when the root *qeḷ*- gives *calācala*- side by side with *carāmi*. Hence, as was inevitable, *calācala*- was referred to a hypothetical root *cal*- (not productive, so possibly only grammatical), but on the other hand in Bk. x. *calācala*- is replaced by *carācara*-. This is some evidence that our view of *cakra*- may be correct.

4. All derivatives from *ḷ*-roots with the suffixes *-ḷo*- or *-ṛo*- would have their *ḷ* represented by *l*, and all derivatives from *ṛ*-roots with *-ḷo*- would have *ṛ* becoming *l*, while *r* would appear in other forms. Hence *sabila*- from *√seḷ*-, *kīlāla*- from *qa^xiṛ*- &c.

5. *-go*- is a common I.-E. suffix, and added to roots in which *l* was not protected by an explosive would cause it to

appear as *l*. Hence *çloka-* beside *çru* from *kley-*, and *çulka-* from an unknown root. Cf. also *ulkā* beside *varc*.

6. The fact that the reduced stage of the root shows *r* in Skt. as representing I.-E. *l* and *r* (aided by the similar fusion of *l̄, r̄ : l̄̄, r̄̄*), would exert a great levelling tendency. Thus the root *peḷ-* would give in Skt. **pal-*, *pr-*, *pur-*, *pūr* for *peḷ-*, *pl̄-*, *pl̄̄-* and *p̄̄* respectively, so that it is not to be wondered at if the isolated *pal-* disappeared before the systematic *par-*. Conversely *pulu-kāma-* and *pulvaghā-* are incorrect.

7. Highly important for the question of the 'sonants,' to which attention is being called by Bechtel, Fennell and other writers, is the distinction which we are able to draw between *əl̄* and *l̄̄*, as when a velar &c. precedes, the one appears to give *il* the other *ir*, cf. *kilāsa-*)(*akirat*. This seems against the 'minimal-vocal' theory; but the subject needs fuller examination.

8. We saw that in *kuliça-* (p. 229) the prefix *ku-* appears to have kept the *l* which becomes regularly *r* in *riç*. Cases like this might—if the sense-connexion were retained—produce *liç-* and *riç-* as doublets.

9. Lastly we saw that decomposition was responsible for the *l* of *āla-* and *nīla-*, and it is not an unfair assumption to suppose that in other cases the same effect was produced by a compound which did not happen to be preserved.

These nine distinct influences tending to modify the rigidity of the laws are the direct result of the conditions which the laws themselves create: they are therefore *a priori* probable and more than suffice to explain the exceptions from the rules which are to be found in R.V. These we have seen with the single exception of the suffix *-la-* to be few in number compared with the large body of examples in favour of the rules. As usual however the study of the exceptions is the most important part of the work, and we are fortunate in having just a sufficient number embodied in R.V. to prove that not only were the above nine tendencies *a priori* probable, but were actually in existence.

§ 6. THE SIXTH PERIOD.

The Sixth Period or classical Sanskrit cannot be adequately treated with the materials at hand. So many roots have been invented by the grammarians that it is necessary to investigate the authority for each of them, before they are accepted in evidence. As far as *l*-roots are concerned the state of things is described by Whitney in a remark which Brugmann quotes as sufficient reason for shelving the subject: "there is hardly a root containing *l* which does not show also forms with *r*; words written with the one letter are found in other texts or other parts of the same texts written with the other" (Whitney, *Skt. Gram.*¹ p. 18, cf. Brugm. *Grds.* i. p. 210).

Now whether the classical Skt. is or is not a direct descendant of Vedic, we have seen enough confirmatory evidence drawn from it to lead us to assume that it had not become separated at the time of the differentiation of *r* and *l*. Accordingly its history was at least parallel with that of Vedic, and all the just enumerated nine causes of change would affect it in the same way. Therefore, allowing for the greater lapse of time, and taking into account the much greater irregularity which we find in R. V. x., we need not wonder if they have power to produce the state of things described by Whitney at the epoch of classical Skt. The reassuring point is this: the converse of Whitney's statement does not hold true, and roots containing *r* do not universally show *l* at some times. This absolutely agrees with the result to be expected from our tendencies, which nearly all favoured the expulsion of *l* by *r* and not *vice versa*.

In this connexion it may be of interest to note that in the fourth edition of Fick's *Wörterbuch*, Vol. I., he assigns European *r* to Skt. *l* in the five following words (and these only, so far as a not too minute perusal has extended):

lelīya-, p. 115.

likha-, p. 115.

loha-, p. 116.

luñc-, p. 119.

kalp-, p. 186.

Of these *lelīya-* and *kalp-* have been already explained. The explanation of *loha-* is the same as that of *lohita-*. *luñc-* 'to tear' is not directly the Latin *runcare*, Lith. *runkti*, but again we have a double type of root: the *ḷ* form has the evidence of *λ'γξ*. This then appears in a form in which the velar retained its rounding. *likha-* again belongs to a doublet *ṛikh-* *ḷikh-*, although there does not seem confirmatory evidence.

We obtain then a guiding principle for proceeding with the liquids even in later Sanskrit. An etymology in which *l* and *r* interchange in accordance with our rules may be looked upon as fairly safe: one in which the rules are violated so that we find Skt. *l* representing I.-E. *ḷ* outside the conditions prescribed may also be considered possible: one in which Skt. *r* represents I.-E. *ḷ* outside the same conditions must be questioned or a double root suspected, and one in which Skt. *l* irregularly represents I.-E. *ṛ* must be established by the strongest possible evidence.

7. MISCELLANEA ETYMOLOGICA.

a. *σμ-* in Greek.

I.-E. *sm* became *zm*. This *zm* became in Greek *-μμ-* and *μ-*. Hence *μία* represents *zmīa* from *√sem*.

But the prefix *s-* still existed: it formed new composita at a later but pre-Homeric date. This *sm* became *σμ* in Gk. possibly by a loss of voice in the *μ*.

For this compare

σμερδ- { αλέος { νός	with	ἀ-μέρδω
σμάω, σμήχω (? σμήρις &c.)		μάσσω (?)
σμαλερός		μαλερός
σμάραγδος		μάραγδος (but perhaps borrowed or cf. μάρ- γοι and μαργαρίτης).
σμαραγέω		μαράσσω
σμάραγνα		μάραγνα
σμαρίς		cf. μαρίνος (? foreign).
σμήνος		
σμηρία (= κισσός Hesych.)		
σμήριγξ		μήριγξ
σμήρινθος		μήρινθος
σμίγδην		μίγδην
σμίκρός		μικρός

σμίλαξ	μίλαξ
σμίλη	
σμίλος	μίλος
Σμινθείς σμίνθος (? foreign)	
σμινύη	
σμυγερός}	μογερός
σμογερός}	
σμοιός	μοιός
? σμοκορδόω	
σμούδρος	μύδρος
σμούλα (? foreign)	
σμυλίχη	
σμούξων	μύξων
σμύραινα	μύραινα
σμυρίζω	μυρίζω
(σμύρις foreign)	
σμύρνα	cf. μύρρα (foreign ?)
σμύσσω	μύσσω
σμύχω	
σμώγω	
σμῶδιξ	
σμώνη	

Possibly this reasoning might be applied to the *σμ*- difficulty. I can't see my way through it at present. July 23. '92.

β. *Gr.* ἀλείφω, *Lat.* *lībo*.

ἀλείφω beside λιπαρός &c. presents difficulties. Can it not be from *leībē* *Lat.* *lībo*?

lībo would then combine two roots, as it also = λείβω. To this the variations in sense between "touch" and "pour" seem to point. *lībum* is a cake 'smeared' with honey. Then perhaps the nature of the initial *l* differed in the two roots. [See p. 198. C.]

γ. *On Iliad* 13. 707.

‘τεμει’ δέ τε τέλσον ἀρούρης.

τέλσον ἀρούρης is also used Σ 544 (cf. 547), where it seems to have the sense of the “extremity” or “boundary of the ploughed land.” It is scarcely possible to give it that sense here whether τέμει or τεμεί be read with the sense of τέμνω. The translations

“It (the yoke) cuts (or will cut) the boundary of the land,”

“It (the furrow) cuts (or will cut) the boundary of the land,”

“The boundary of &c. cuts (or will cut) the furrow (or the yoke),”

are all either nonsense or otiose.

It may perhaps be that τέλσον is to be taken differently.

It answers sufficiently closely to Lat. *tellus* stem *tellūs*-, the suffix of which is very difficult to analyse; but of which the root may easily be *tels*-. In that case τέλσον ἀρούρης is the ‘dry’ surface of the field.

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G. KELTIC.

H. TEUTONIC.

(A.S.=Anglo-Saxon, E.=English, E.E.=Early English, G.=Gothic, O.H.G.=Old High German, M.H.G.=Middle High German, Mod.H.G.=Modern High German.)

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